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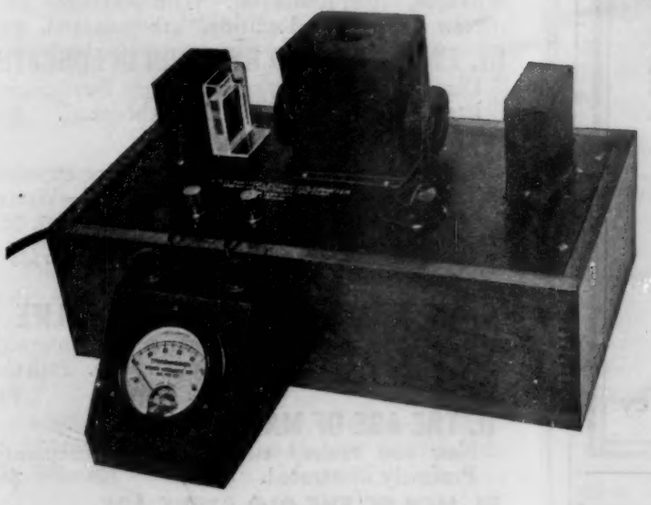

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SCIENCE

VOL. 75 FRIDAY, APRIL 22, 1932 No. 1947

Statistical Mechanics and the Second Law of Thermodynamics: DR. P. W. BRIDGMAN 419

Obituary:

Charles Sheldon Hastings: PROFESSOR FREDERICK E. BEACH. *Recent Deaths* 428

Scientific Events:

A New Tuberculosis Building at the Pasteur Institute; The X-ray Unit of the Hospital of the University of Michigan; Exhibit of the Philadelphia Mineralogical Society; The American Medical Association; Fellowship Awards in the Sciences at Yale University 430

Scientific Notes and News 433

Discussion:

The Analysis of Electromyograms: DR. H. DAVIS, DR. A. FORBES and L. GARCEAU. *The Pine Forests of Costa Rica:* PAUL C. STANDLEY. *The Soaring of Turkey Buzzards:* PROFESSOR G. S. DODDS. *On the Early History of Platinum:* E. P. C. MEYER 436

Reports:

The Incorporation of Scientific Societies: DR. BURTON E. LIVINGSTON, PROFESSOR JAMES B. OVERTON and DR. WALTER THOMAS 438

Scientific Apparatus and Laboratory Methods:

Apparatus for Determination of CO_2 and O_2 of Respiration: DR. M. H. HALLER and DR. D. H. ROSE. *Vegetable Peptone Agars for Quantitative Work with Lactobacillus Acidophilus:* DR. FREDA M. BACHMANN and PROFESSOR W. D. FROST 439

Special Articles:

Cure of the Cachexia Following Hypophysectomy by Administration of the Growth Hormone: PROFESSOR HERBERT M. EVANS, PROFESSOR KARL F. MEYER, RICHARD PENCHARZ and DR. MIRIAM E. SIMPSON. *The Transformation of Serum Albumin into Serum Globulins:* A. FISCHER. *Periodical Properties of Atomic Nuclei:* G. I. POKROWSKI 442

Science News 8

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STATISTICAL MECHANICS AND THE SECOND LAW OF THERMODYNAMICS[1]

By Dr. P. W. BRIDGMAN

HARVARD UNIVERSITY

ONE thing that has much impressed me in recent conversations with physicists, particularly those of the younger generation, is the frequency of the conviction that it may be possible some day to construct a machine which shall violate the second law of thermodynamics on a scale large enough to be commercially profitable. This constitutes a striking reversal of the attitude of the founders of thermodynamics, Kelvin and Clausius, who postulated the impossibility of perpetual motion of the second kind as a generalization from the uniformly unsuccessful attempts of the entire human race to realize it. Paradoxically, one very important factor in bringing about this change in attitude is the feeling of better understanding of the second law which the present generation enjoys, and which is largely due to the universal acceptance of the explanation of the second law in statistical terms, for which Gibbs was in so large a degree responsible. Statistical mechanics reduces the second law from a law of ostensibly absolute validity to a statement about high probabilities, leaving open the possibility that once in a great while there may be important violations. Doubtless another most important factor in present scepticism as to the ultimate commercial validity of the second law is the discovery of the importance in many physical phenomena of those fluctuation effects which are demanded by sta-

[1] The Ninth Josiah Willard Gibbs Lecture, delivered at New Orleans, December 29, 1931, under the auspices of the American Mathematical Society, at a joint meeting of the society with the American Physical Society, and Section A of the American Association for the Advancement of Science.

tistical mechanics. It is very hard indeed for one who has witnessed the Brownian motion for the first time to resist the conviction that an ingenious enough engineer might get something useful out of it, and I have no doubt that many in this audience have tried their own hand at designing such a device, and I also have no doubt that their success has been discouragingly nil.

There are other aspects also of the statistical point of view which have become prominent in the last few years, as, for example, the speculations of Eddington on time's arrow and on the meaning of time in general, the speculations of Lewis as to completely reversible time, all the recent concern and new notions about the destiny of the universe as a whole, in which thermodynamic arguments play a most important part, and it is of course notorious that the notions of probability, which are fundamental in statistics, are at the very bottom of wave mechanics.

It is evident, therefore, that the statistical point of view entails consequences important both conceptually and practically. In the hope of helping a little to a better understanding of the situation I propose to-day to examine a few of the implications and consequences of the statistical point of view. The program is a very modest one, and I hope you will have no expectation of a final solution of any of these difficult questions; my primary purpose is to awaken a more vivid self-consciousness of what the situation actually is. I shall be mostly concerned with the classical statistics, and shall have less to say about questions raised by wave mechanics; we shall find that the questions raised by the classical point of view are sufficiently fundamental. I shall throughout adopt the point of view that I have called *operational,* that is, I shall seek the meaning of our statements and concepts by trying to analyze what it is that we do when we are confronted with any concrete physical situation to which we attempt to apply the concept or about which we make the statement.

It is in the first place most important to realize that the statistical method, in which the notions of probability are fundamental, has, when applied to the understanding of physical situations, certain inherent, unique, logical characteristics, so that any account which statistics can give of physical phenomena must have an entirely different aspect from that of such a method of approach as classical mechanics, for example. The reason for this is that probability is not a notion which can be applied to concrete individual events, whereas we demand that we understand, or predict, or control, the individual event. I demand to know what will happen when I throw this particular stone in the air, or explode this particular charge in the cylinder of a gas engine. Ordinary mechanics gives an unequivocal answer, and in general the explanations of ordinary mechanics make direct contact with just such concrete individual physical events. But the notions of probability have no such application to individual events, and in fact the notion of probability is meaningless when applied to an individual event. The proof of this is given by mere observation of what we do in applying the notion of probability. Suppose that I show you a die and remark that I intend to throw it in a minute. You volunteer the information that the probability is one sixth that the throw will be a six. I am skeptical and ask you to justify your statement by pointing out the property of the event, when it takes place, that can be described as a probability of one sixth for a six. I then make the throw and get a six. What possible characteristic has this single event that can justify your statement? Your statement has immediate meaning only when applied to a long sequence of events, or when applied to the *construction* of the die and the method of throwing it. Even when applied to a sequence of events there is always an unbridgeable logical chasm thwarting a precise application of the notion of probability to any actual sequence. Consider, for example, the classical example of tossing a coin. In practice our first concern is to determine whether the coin is a fair coin, that is, whether it is equally likely that heads or tails appear. Suppose that we make a million throws, and find the excess of one or the other not to be more than 1,000. Then we are likely to say that the coin is fair. But, logically, we are bound to recognize that the coin may have been loaded so that heads were, perhaps, nine times as likely to appear as tails, only that we had happened on one of those excessively rare sequences in which as many tails appeared as heads. Rigorously, there can be no method by which we can be sure that all our past experience has not been one of these excessively rare sequences which logically we are bound to recognize as possible in any statistical assembly.

Passing over these logical difficulties in applying precisely the notions of probability to any actual physical situation, it is evident, I think, that, when applied to individual events, probability can have only a secondary or derived meaning. I believe that an examination, as the operational point of view prescribes, of what one does, will show that the meaning to be ascribed to the probability of individual happenings is to be found in the rules of the *mental* game that one plays in thinking about and trying to understand the individual events. This has important consequences when we attempt to use the notions of probability in building physical theories. We must recognize in the first place that any physical theory demands the construction of some sort of model. Now

any model involving notions of probability is of necessity more remote from the physical situation and more esoteric in character than the more usual and naïve models, such as are offered by ordinary mechanics. For any statistical model involves conventionalized events to which the notions of probability are by definition applicable, although the notion of probability does not apply at all to the concrete physical events which are the counterpart of the events of the model. It is therefore not surprising that the connection between the properties of the statistical model and the corresponding physical system is somewhat different from the connection in the more ordinary sorts of model.

It would be possible to digress considerably here to discuss the properties which we demand in our models, and the uses to which we put them. The subject is fairly obvious, however, and I believe we can safely assume an understanding of the essential features. The least exacting demand that we make of a model is that it serve as a calculating device, by which we can predict actual physical happenings, and for this purpose any sort of consistent correspondence between the model and the physical happening is satisfactory. The simplest way in which the statistical model can satisfy this simplest and minimum demand is evidently that actual happenings in the physical system shall correspond to high probabilities in the statistical model. This furthermore seems to be the only possibility and all that we can do; in the model there are no certainties, only probabilities, some of which, it is true, may be very close to unity, whereas in the physical system there are invariable happenings, as for example, a cake of ice always melts when it is heated above the melting point, or the external atmosphere always rushes into an exhausted electric light bulb when it is cracked. On the other hand, it is most natural to say that low probabilities in the model correspond to infrequent occurrences in the physical system. But to go further and specify exactly how close to unity we shall demand that the probability be that is to correspond to an invariable happening, is not so easy, and there seems to be a certain unavoidable fuzziness here defeating every endeavor to make sharp connection between the model and actuality. There is a still greater difficulty in giving a precise physical significance to events in the model of low probability; we shall return to this question. For the present the important point for us is that any statistical model is in peculiar degree purely a paper and pencil model, and peculiar limitations may be expected in the use of such a model.

If the only demand that we put on the model were that it should serve as a computing device, the situation would be comparatively simple and could be quickly dismissed. But as a matter of fact we make the more exacting demand that the model enable us to understand the physical situation, and to this end we demand that there be a further correspondence between the properties of the model and of the physical system. Since we do not usually make the extreme demand that the model enable us to understand all the physical properties of the system at once, we usually do not demand that there be an exhaustive correspondence between the properties of the physical system and those of the model, but we are satisfied with a correspondence of those properties only which are pertinent for our immediate purposes. Thus for the discussion of the thermodynamic properties of a perfect gas, a model is usually sufficient in which the molecules of the actual gas are replaced by perfectly elastic spheres or ellipsoids, although such a model gives no hint of the optical properties of the actual gas. It is curious, however, that even for thermodynamic purposes we would not be satisfied with a model in which the number of fictitious molecules is not equal to the number of actual molecules which various other sorts of physical evidence lead us to ascribe to the actual gas.

If, now, we are attempting to find a model for the thermodynamic properties of a gas, we see that the accepted models which satisfy these additional requirements go far beyond the original demands, for in such models we encounter all the phenomena of fluctuations. Strictly, such a model never comes to equilibrium, and can not therefore possess any property which strictly corresponds to temperature as defined classically in terms of equilibrium states. The remarkable fact, of course, is that the fluctuations of the model were found to correspond to fluctuations in the physical system, as shown by the Brownian motion, and in consequence we have now come to recognize that temperature is physically only an approximate concept, instead of the exact concept originally assumed in thermodynamics. This gives us at once one possible method of dealing with the second law and its apparent violations. If we choose to formulate the second law by the statement that dQ/T is an exact differential, then by its very form it applies only to those situations to which the temperature concept applies, and since the temperature concept never exactly applies to any physical situation, we are left with a law which may be rigorously exact in the limit, but which applies to no actual situation. This method of meeting the situation may perhaps be satisfactory to the pure logician, but to the individual interested in filling his pockets by bootlegging entropy, such considerations will appear as uninteresting and as ineffectively legalistic as the restrictions which the more ordinary sort of bootlegger fails to recognize.

It is probably not possible to set up a mechanistic model of a purely thermodynamic system, for the thermodynamic system knows no details, but only pressure and volume and temperature, whereas it is the essence of a mechanical model that it contain details which can have no counterpart in the physical system in so far as it is purely thermodynamic. This prepares us to recognize that the concepts of thermodynamics have no absolute validity, but are relative to the operations, and in particlar to the scale of the operations which we use. Thus a fluid in turbulent motion may have a temperature from the point of view of a thermometer with a bulb several centimeters in diameter, but may have no temperature from the point of view of a minute thermo-couple such as biologists have recently used in probing the interior of single cells. Or again, the entire body of phenomena to which the so-called third law is applicable would immediately appear in a different aspect and the third law would no longer be applicable if the operation of taking atoms apart and recombining them was added to the other permissible operations of a more conventional character. In recent papers Dr. David Watson has discussed some of the consequences of a recognition of some of the other relativistic characteristics of the entropy concept.

There seem to be two diametrically opposite and equally natural reactions to an appreciation of this situation. The first reaction is that of the younger generation, part of which, at least, expects to discover some day in the realm of microscopic operations the possibility of a profitable entropy bootlegging business, and there is, secondly, the reaction of the other school, which is convinced that the second law involves something absolutely fundamental, and that any formulation in terms involving relative magnitudes or permissible operations can be only an incomplete formulation.

In the endeavor of the second school to find a more fundamental formulation it is natural to attempt to capitalize the striking success that the statistical point of view has already had in dealing with the phenomena of fluctuations. One recent and well-known attempt in this direction is that of Eddington to appraise the second law in terms of what is essentially a shuffling process. Eddington sees in the universal tendency for entropy to increase, or for a system to pass from a less to a more probable configuration, the analog of what happens when a pack of cards is shuffled. Now although there may be strong points of analogy between these two processes, it seems to me that there are also essential differences, and that the analogy on the whole is not a happy one. Shuffling acquires meaning only when we are able to apprehend the cards as individuals by marking them so that we can identify them as individuals, but in such a way that there shall be absolutely no effect on the shuffling process. The picture that Eddington had in mind was a pack of cards freshly received from the manufacturer, arranged in suits and by rank in the suits, and then shuffled and losing all trace of its original arrangement. But, as G. N. Lewis has remarked, the arrangement of the cards in suits is one of entirely arbitrary significance; from the point of view of some other game than whist the initial arrangement was already a completely shuffled arrangement as well as all the subsequent ones. Or we may look at it in another way. Imagine an infinite sequence of deals, the cards being partially shuffled between each deal, and suppose that a complete record is kept of all the hands. Somewhere in this sequence there will be deals in which the cards are distributed among the hands by suit and the arrangement in each hand is by rank. If one examines the record it will be found that as one proceeds away from the exceptional deal, both forward and backward in the sequence, all trace of the regular arrangement becomes obliterated. That is, shuffling with respect to any configuration selected arbitrarily as of special significance is symmetrical with respect to past and future time, and the analogy with the thermodynamic situation disappears. There are other difficulties with the shuffling point of view. We have seen that shuffling has meaning only when the cards can be identified as individuals. But if the cards can be handled as individuals, they can be unshuffled, as any whist player will demonstrate to you within five seconds of picking up his hand. To see in the shuffling process something analogous to the inexorableness of the increase of entropy in nature involves the thesis that, although it may be possible to identify the elements in a physical situation, there is some restriction in nature which prevents us from treating these elements separately and sorting them out. This, it seems to me, is a hard doctrine. There is no suggestion of such a state of affairs in any picture offered by the classical mechanics, and classical mechanics was all that Eddington had in mind. Until the reason is elucidated for this surprising inability of ours to handle what we can see, I believe that the shuffling analogy must be judged only to obscure the situation rather than to clarify it.

At first glance, the possibility of understanding the strange restriction against touching what we can see is even more remote from the wave mechanics point of view than from the classical, for seeing is now to be considered as a kind of touching, namely, touching with a photon. It may well be, however, that we have here the key to the ultimate solution, for it is not sufficient merely to touch, but the touch must also control; such a kind of touch doubtless requires the coop-

eration of a great many photons, and thus will be less possible of attainment than the touch by a single photon which is sufficient for recognition. But this point of view we do not follow further here.

Not only does Eddington see in shuffling the complete analogy of the inexorable increase of entropy, but he sees something even more fundamental, namely, an explanation of the properties of time itself, and in particular why it is that time is unsymmetrical and flows only forward. This he has expressed by saying that the increase of entropy of the universe is what it is that gives direction to time's arrow. This conception deals with such fundamental matters and has been hailed in so many quarters as being of such unique profundity, that we may be pardoned for stopping for an examination of what is involved. Eddington sees the crux of the matter in an essential difference between the equations of ordinary mechanics, including electrodynamics, and thermodynamics. The equations of ordinary mechanics and of electrodynamics, which express what Eddington calls primary law, are of such a character that the differential of time may be reversed in sign with no change in the equations, as, for example, in the equation for a falling body, $d^2s/dt^2 = -g$. It is evident enough that the equation is unaltered if the sign of dt is changed; the question is what is the significance of this observation? The significance that Eddington ascribes to it is that the equation is unaffected by a reversal of the direction of flow of time, which would mean that the corresponding physical occurrence is the same whether time flows forward or backward, and his thesis is that in general there is nothing in ordinary mechanical occurrences to indicate whether time is flowing forward or backward. In thermodynamic systems, on the other hand, in which entropy increases with time, time enters the differential equation as the first derivative, so that the direction of flow of time can not be changed without changing the equation. This is taken to indicate that in a thermodynamic system time must flow forward, while it might flow backward in a mechanical system.

As thus expressed there seems to be considerable vagueness about some of the ideas; this vagueness I find in Eddington's original formulation. Such vagueness is natural and perhaps inevitable in a popular exposition, but if we are to understand from the operational point of view what is involved here, we must try to be more precise. Careful analysis has not yielded to me more than the following as an exact statement of what is involved. Imagine a closed system, and an assistant with a note book in which at a given instant of time he notes all the data necessary to characterize completely the configuration of the system. At a later instant of time he records the corresponding data in another note book. He then gives us the note books and we try from a study of them to determine which set of data was recorded at the earlier instant of time. If the system was a thermodynamic system we can make the decision, because the entropy increases with time, but if the system was a mechanical system we can not decide which note book was used first, because examples could be set up for either one or the other. Eddington expresses this difference by saying that the direction of flow of time has no significance in the mechanical system.

It is, of course, true that the differential equation of the mechanical system is differently constructed from that of the thermodynamic system, but the significance of the difference does not need to be formulated as does Eddington. We must in the first place remember that the equation of the mechanical system, for example, $d^2s/dt^2 = -g$, applies not only to a single system—that is, to a single falling body—but applies as well to a family of systems. The equation has the property that, corresponding to every specific system, with its particle at a definite point moving with a definite velocity at a definite instant of time, another system is possible with its particle in the same position at the same instant, but with a negative velocity. This is because the equation is of the second order, and gives on integration two constants, which may be so adjusted as to give any position and any velocity at any instant of time. Imagine the second system set up; as time goes on it will trace out in reverse sequence the positions of the first system, as may be seen from the equation itself, which may be written

$$\frac{d^2s}{dt^2} = \frac{dv}{dt} = \frac{d(-v)}{d(-t)} = -g.$$

It is this fact which makes it impossible to decide, in our example above, which note book was used first, because there is no way of telling from the entries alone whether they applied to the first or the second system. But in no case is there any question of time flowing backward, and in fact the concept of a backward flow of time seems absolutely meaningless. For how would one go to work in any concrete case to decide whether time were flowing forward or backward? If it were found that the entropy of the universe were decreasing, would one say that time was flowing backward, or would one say that it was a law of nature that entropy decreases with time? It seems to me that in any operational view of the meaning of natural concepts the notion of time must be used as a primitive concept, which can not be analyzed, and which can only be accepted, so that it is meaningless to speak of a reversal of the direction of time. I see no way of formulating the underlying operations without assuming as understood the notion of earlier or later in time.

Lewis, in a recent paper, "The Symmetry of Time in Physics," adopts a point of view in some respects similar to that of Eddington, although Lewis would certainly disclaim more than very partial agreement with Eddington. Lewis speculates about the fundamental significance of the symmetry of time in physics. His point of view takes its origin from the four-dimensional representation of events employed in relativity theory, and the consequent reduction of all propositions in kinematics to propositions in four-dimensional geometry, rather than from the form of the equations of mechanics, as in Eddington's theory. But both neglect what I believe to be the most important aspect of the situation. Both the equations giving the motion of the system and the four-dimensional representation of the motion are only a small part of the story. The equations are without significance unless the physical operations are also defined by which numerical values are assigned to the various symbols of the equations. For instance, in treating a falling body, we need in addition to the equation itself a set of directions for the use of the equation, in which it is set forth, among other things, that s is the number obtained by making with a meter stick certain manipulations connected with an arbitrary origin and the instantaneous position of the falling body. Similarly, in the four-dimensional representation, we must know the physical operations by which the numerical values of the coordinates are obtained which go into the four-dimensional diagram. If one examines the operations by which meaning is given to the symbols which occur in the equations or to the coordinates in the geometrical representation it will be found that the time concept has to be assumed as primitive and unanalyzable, for the operations essentially assume that the operator understands the meaning of later and earlier in time. For example, in order to find the velocity of a particle, one has to observe its position at some one instant of time and combine with this in a prescribed way the result of another observation at a later instant. If one does not intuitively understand what is meant by a later instant, there is no method of formulating the operations. The same situation is involved in specifying a thermodynamic system. One of the variables is the temperature; it is not sufficient merely to read at a given instant of time an instrument called a thermometer, but there are various precautions to be observed in the use of a thermometer, the most important of which is that one must be sure that the thermometer has come to equilibrium with its surroundings and so records the true temperature. In order to establish this, one has to observe how the readings of the thermometer change as time increases.

This point of view, that the schedule of operations by which the symbols acquire meaning is as important a part of the physical situation as the relations which are found to hold between the symbols themselves, has an important bearing on a very widely spread tendency in modern physics and science in general to see nothing as significant except the relations, and so to reduce all science to a kind of topology. It is this point of view that is at the bottom of Einstein's philosophy when he says, for example, that all that is observed is a series of space-time coincidences, and which Eddington expresses by saying that nature may be reduced to a series of pointer readings. If one grants that the ultimate object of physics is to establish a certain sort of relation between us and the world of our senses, to speak with a certain monstrous naïveté, I do not see how it is possible to discard as irrelevant the fact, for example, that the fourth coordinate in the four-dimensional geometry of relativity has to be obtained by an entirely different sort of operation from the other three coordinates, or to regard the entire situation as exhaustively characterized by the relations between the numbers, irrespective of how they are obtained.

We return now to a further consideration of our statistical model and the methods by which we shall establish a correspondence between its properties and those of the physical system. Hitherto we have been gratifyingly successful; events in the model of overwhelmingly high probability correspond to invariable happenings in the physical system, and less common events in the model, such as fluctuation phenomena, are found to be prophetic of a previously unsuspected new domain of physical effects, typified by the Brownian motion. Encouraged by this success, it is natural to think that we have got hold of something real, whatever that may mean, and to push our scheme of correlation to the logical limit, and say that all the excessively rare events corresponding to low probabilities in the statistical model are correlated with corresponding rare events in the physical system. Now it is a consequence of the fundamental assumptions which have gone into the usual statistical model, namely, that all elementary configurations are entirely independent of each other, so that the probability of any configuration is to be calculated by purely combinatorial methods from the relative number of ways in which the configuration can be realized, that there is some chance of the occurrence of any configuration, no matter how unusual its properties. This would mean that in the corresponding physical system any configuration whatever, compatible with the fixed conditions, would occur occasionally, as, for example, the gas in a box will occasionally automatically all collect itself into one end. This conclusion is indeed taken literally by many experts in statistical mechanics, and

in the literature statements are not uncommon, such, for example, as that of Bertrand Russell in a recent magazine article that if we put a pail of water on the fire and watch it for an indefinite time, we shall eventually be rewarded by seeing it freeze. It seems to me that there are a couple of objections that can be made to the conventional treatment of rare occurrences, which I shall now examine.

The first difficulty is with the technical method of calculating the chances of observing a rare configuration, and is concerned only with the model itself, and not with the physical application of the results of the calculations. In computing the chance of any configuration, it is always assumed that the elements of the statistical model are without influence on each other, so that the chance of a given configuration is given by merely enumerating the number of complexions corresponding to the given configuration. For example, in the kinetic theory of gases it is assumed that the location of any molecule and its velocity is, except for the restriction on the total energy and the total volume, independent of the location or the velocity of any or all of the other molecules. It may be proper enough to postulate this for the model, but we know that it can not rigorously correspond to the physical system, for the molecules of a gas do interact with each other, as shown by the mere fact that they conserve their total energy, and the transmission of energy from one molecule to another takes place only at a finite rate, so that if, for example, at one instant all the velocity were in a single molecule, we would find that immediately afterward only molecules in the immediate vicinity had any velocity. This means that the assumption of complete independence must be recognized to be only an approximation, and some way of handling this approximation must be devised. The method usually adopted is to cast the problem in the form of inquiring how many observations must be made in order that the chance of observing the desired rare configuration may be one half, for example, choosing the time between observations so long that at each observation all appreciable trace of the previous configuration shall have been obliterated, so that the assumption of independence may apply. The point now is this: the time that one has to wait for the probable obliteration of all traces of a previous configuration becomes longer the rarer the previous configuration; obviously it takes longer for a gas to efface all trace of having been all concentrated in one half of its available volume than to efface the traces of a small local concentration. The situation is, therefore, that not only must we make an increasingly large number of observations in order to hope to witness a rare configuration, but the interval between our observations must also get longer. It is merely the first factor which is usually considered; when both factors are considered it is not at all obvious that the process is even convergent. This point should be subjected to further examination.

There is another difficulty connected with the mere calculation of the probability of rare occurrences presented by quantum theory. All classical calculations assume that the molecules have identity. But the uncertainty principle sets a limit to the physical meaning of identity. It is not possible to observe the position and velocity of any molecule with unlimited precision, but there is a mutual restriction. After an observation has been made, the domain of uncertainty in which the molecule is located expands as time goes on. If the domains of uncertainty of two molecules overlap, then the identity of the molecules is lost, and a subsequent observation will not be competent to decide which molecule is which. The only way of maintaining the identity of the molecules is by making observations at intervals so frequent that the domains of uncertainty have not had time to overlap. But this time is obviously much shorter than the time between observations demanded by the requirement that all trace of the previous configuration shall have been wiped out. Furthermore, the act of observation, by which the concept of identity acquires meaning, alters in an uncontrollable and unpredictable manner the motion of the molecules, whereas the statistical treatment requires that the molecules be undisturbed between successive observations. It seems, therefore, that the physical properties of actual molecules as suggested by quantum theory are different from those of the molecules of the model, and this would seem to demand at least designing new methods of calculating the chances of rare occurrences.

Apart from these objections, which may be met by the discovery of new theoretical methods of attack, it seems to me that the most serious difficulty with this question of rare states is met in the process of transferring to any actual physical system conclusions based on a study of the corresponding model. Suppose, for example, that we are discussing the problem of the tossing of some particular coin. If the coin is a fair coin, that is, if the chances of heads and tails are even, then our statistical model consists merely of a sequence of one or the other of two events, each of which is as likely to occur at any time as the other, absolutely independently of what may have happened elsewhere in the sequence. The theoretical discussion of this model is very easy, and we are convinced that conclusions drawn from a discussion of the model will apply to the tossing of the coin, always provided that the coin is a fair coin. As a particular problem

we may consider the chance of throwing heads ten consecutive times. The chance of this is $(1/2)^{10}$, or 1/1024, which means that in every 1,000 consecutive throws the chances will be roughly even that there will be somewhere a sequence of 10 heads.[2] But 1,000 throws are a good many, and it may be that we have never made so many throws, and are content merely to make the prediction that if some one else should make so many throws it would be found to be as we say. But suppose that some one questions the fairness of the coin, and says that he has reason to think that there is a bias of 10 per cent. in favor of throwing tails, so that the chance of a head at a single throw is only 0.45 instead of 0.50. We find now on making the calculation that we shall have to make roughly 10,000 throws in order to have an even chance of getting a sequence of 10 heads; and, in general, that slight imperfections in the fairness of the coin make very large differences in the chance of rare occurrences. In view of this, we feel that it behooves us to make some objective test of the fairness of the coin before we venture to publish our prediction that we are likely to get a sequence of 10 heads in 1,000 throws. We make the most direct test possible by appealing to the fundamental definition of fairness, which is that in a large number of throws the ratio of the number of heads to tails tends to equality. But how many throws are necessary to establish such an equality with satisfactory assurance! There is another theorem here, namely that in n throws the chances are even that we shall have an excess either of heads over tails or of tails over heads of $0.6745\ n^{1/2}$. Neglecting the numerical factor for our rough purposes, this means that if we make a hundred throws the chances are nearly even that the number of heads is somewhere between 45 and 55. To establish the fairness of the coin we would have to make a considerable number of 100 throws at a time and observe whether or not the number of heads clusters between 45 and 55. If, on the other hand, there is a 10 per cent. bias in favor of tails, the number of heads will cluster between 40 and 50. The precise number of sequences of 100 throws at a time necessary to convince us that there is no 10 per cent. bias in favor of tails obeys no definite criterion, but it is certainly of the order of ten or more, which makes 1,000 or more throws altogether. But this was the number of throws necessary to obtain one of the rare sequences of 10 heads.

The conclusion from all this is plain; in order to establish with sufficient probability that the actual physical system has those properties which are assumed in estimating the frequency of rare occurrences it is necessary to make a number of observations so great that the probability is good that the rare occurrence has already been observed. In other words, purely logical statistical considerations never can justify us in predicting events so rare that they have never yet been observed. A pail of water has never been observed to freeze on the fire; statistical considerations give us no warrant whatever for expecting that it ever will. Such predictions can be made only on the basis of considerations other than statistical. Thus in the case of the coin, an exact measurement of its dimensions and of the degree of homogeneity of its metal might convince us that the chances of heads and tails were even, because of our knowledge of the laws of mechanics. But when we come to the molecules of a gas or the elements of other physical systems to which the statistical method of treatment is usually applied, we see that there is no method of independently handling the elements, so that the statistical method of testing the validity of our assumptions is the only possible one. This is a most natural situation, because if we were capable of dealing with the elements of the physical system as individuals we could apply more powerful methods than the statistical. Incidentally we may remark how very insensitive the statistical method is in studying elemental properties; this is shown by the example of the coin above, where we had to make something of the order of 1,000 throws to establish an asymmetry of 10 per cent. In many cases, however, the statistical method is doubtless the ultimate and the only method.

Another of the applications of statistical ideas in which there has always been much interest, and especially lately, is to the problem of the ultimate fate of the universe. It is a very common opinion that the second law, in its original classical form, demands the ultimate heat death of the universe, because of the inexorable increase of entropy to a final maximum, when all temperature differences shall have been wiped out. The chief mechanism in the ultimate equalization of temperature is obviously the radiation that is continually emitted by the stars. The human mind has, however, shown itself curiously unwilling to accept the prospect of a heat death, and there have been a number of attempts to avoid such an unwelcome conclusion. At least two of these, somewhat similar to each other, are based on the statistical interpretation of the second law. The first of these utilizes the theorem that in a closed mechanical system any configuration, once experienced, is bound to recur after the lapse of sufficient time. According to this view, the universe endlessly goes

[2] I am much indebted to Mr. H. M. James for a rigorous solution of the interesting problem involved here. He finds that between 1,422 and 1,423 throws are necessary for a 0.5 chance of ten or more consecutive heads.

through cycles of repetition, the so-called Poincaré-Zermelo cycle, of prodigious but calculable duration. The obvious objection to this picture is that in order to realize a Poincaré-Zermelo cycle the laws of classical mechanics would have to be satisfied with an exactness quite fantastic, hopelessly beyond the possibility of direct or indirect verification. The second attempt to make satistics avoid the heat death rests on the theorem that a statistical assembly is never in complete equilibrium, but is always subject to fluctuations, and these fluctuations may attain any intensity if we only wait long enough. The present state of the universe is then to be regarded only as a fluctuation, with the possibility that similar fluctuations may recur in the future. The difficulty with this point of view is the excessive rareness of the sort of fluctuation corresponding to the present state of the universe compared with the approximate dead level of the heat death. The previous considerations apply; this is one of those configurations so rare that one has no right to predict its occurrence unless it has been previously observed. One might predict from purely statistical considerations the occurrence of such a fluctuation in the future if one were sure that one were observing such a fluctuation now. But where is the evidence for this? According to the astronomers the fluctuation has been taking the last 10^{16}, or perhaps now 10^{10}, years or so to smooth itself out to its still considerable roughness, and there is certainly no evidence that before 10^{16} years ago the entropy was decreasing instead of increasing.

There are other objections to an application of the second law to the entire universe. The original formulation of the second law was, of course, restricted to isolated systems. By what logical right can the argument be extended to the entire universe? A natural reply is that relativity theory seems to demand that the universe is finite, so that the whole universe becomes the sort of isolated system demanded by the classical formulation. But I believe that examination will nevertheless show a very important difference between the smaller and the all-embracing closed system. Statistical mechanics, if it is to avoid the difficulties already discussed when applied to any individual physical situation, must make the assumption of molecular chaos. But what in the physical situation gives rise to molecular chaos? If we imagine a gas, for example, in a perfectly reflecting enclosure, and suppose that the molecules are perfectly elastic spheres, then, according to the classical picture, every collision takes place under perfectly definite conditions, so that a mathematician of sufficient power could compute backward from the present configuration to the configuration at any past time, as, for example, when a partition might have been removed from the middle of the compartment. This sort of condition certainly can not be described as molecular chaos. But the walls are molecular in structure, so that the reflection of the individual gas molecules follows no definite rule. If we regard the molecules in the wall as part of the external universe, and if there is no coordination between the motion of the molecules of the wall and what is taking place in the gas because of the enormous magnitude of the external universe compared with the gas inside, then a physical reason justifying the assumption of molecular chaos is at once apparent. When the entire universe is considered there can be no such justification as this for assuming molecular chaos, but the whole course of events must, from the classical point of view, run a rigorously deterministic course, to which statistical considerations do not apply. It may be objected to this argument that a gas in contact with its walls in the way described above is not an isolated system. It is, of course, not completely isolated, but it is nevertheless as far as the thermodynamic requirements go, which are concerned only with transfers of energy and of heat from the outside. Complete isolation would seem to be incompatible with molecular chaos.

It was intimated at the beginning of this discussion that the heat death is supposed to be a consequence of the second law, and that the continuous enormous radiation into empty space of the stars is the most striking manifestation of this tendency. This point of view sees in the emission of every photon by a star part of the inexorable increase of entropy. I believe, however, that this is fallacious, that the relations are different, and that the heat death with which we are confronted as a consequence of continued radiation is an affair of the first law, not of the second, and will take place when all the energy of the universe has been radiated away, not merely when the energy is uniformly distributed. Elementary considerations justify this contention. Consider a body radiating into empty space. It is continually dropping in temperature and losing in energy. The emission of radiation therefore decreases the entropy of the radiating body. Consider next a body in thermal equilibrium with its surroundings; since it is in equilibrium its entropy is constant, and furthermore it absorbs as much radiation as it emits. Absorption of radiation, therefore, increases the entropy of the absorbing body. This is sufficient to give a straightforward account of entropy changes in radiation problems. Consider two bodies confronting each other in a cavity at constant temperature. A photon leaves one of the bodies, decreasing the entropy of that body. During the passage of the photon across the space separating the two bodies the entropy is to

be thought of as associated with the photon and residing somewhere in the intermediate space. When the second body absorbs the radiation, the photon with its entropy disappears from empty space and increases the entropy of the absorbing body. At all stages of the process the entropy of the entire system is unchanged. But now suppose that one body radiates to another at lower temperature. This process is irreversible and is accompanied by an increase of entropy. The first two steps of the process, emission of the photon and passage across the intermediate space, are the same as before, and are therefore accompanied by no net change of entropy. It is only the last stage of the process, absorption by the body at lower temperature, that can give the uncompensated increase of entropy. The mechanism of this increase is to be found in a diffusion of the energy of the photon into the greater number of degrees of freedom corresponding to the lower temperature. Actually, this argument is over-simplified, and must be modified by a consideration of the distribution of the photons through the spectrum, but the details of this point of view need not concern us further. The immediate point for us is that emission of radiation into empty space is not an entropy changing process; the increase of entropy can occur, if it occurs at all, only during the act of absorption.

But what physical evidence have we of the absorption of the light radiated by the stars? To save the situation we must postulate absorption under completely unknown conditions. But is the assumption of such unknown conditions in regions so terribly far beyond access to us in order to save the second law any easier as an intellectual feat than the assumption of other unknown conditions which would defeat the second law? Are we not completely in the dark here, and had we not better admit it?

Finally, I briefly summarize what I believe to be the principal results of this analysis and indicate the possible lines of future progress. The most important result will be, I hope, a keen realization that in using statistics we are only using a paper and pencil model, which has logical difficulties within itself and difficulties of application to concrete physical situations which are very much greater than the corresponding difficulties with more ordinary sorts of model. Some of these difficulties I believe can never be surmounted, so that the statistical model can never be satisfactorily used by extrapolation either into remote epochs of time, to predict rare events, or into remote reaches of space, to give us an idea of the course of universal evolution. Our model has not given us a satisfactory answer to our initial question as to the possibility of commercially profitable violations of the second law. The answer to this question will probably be found when the wave mechanics point of view has been completely worked out. Some of the other logical difficulties of the classical statistics, I believe, will also be surmounted by the adoption of the wave mechanics point of view, which assumes probability to be a primitive property of the elements of the model, rather than an emergent property resulting from the cooperation of great numbers. Before, however, the wave mechanics thesis of the primitive character of the notion of probability can be accepted, much more experimental work is necessary. If the thesis is supported, as seems probable from the evidence now at hand, this will constitute to a certain extent a defeat of the purpose of the classical statistics, which was to *explain* why many physical assemblages of large numbers of elements obey the rules of probability. But even granted that the primitive character of the notions of probability acquires an experimental verification, it seems to me that some of the logical difficulties will persist, justifying a doubt as to the possibility of ever setting up a logically completely satisfying correspondence between our models and our experience.

OBITUARY

CHARLES SHELDON HASTINGS

WITH the passing of Professor Hastings, America has lost its foremost expert in optics, and one to whom astronomy owes a profound debt.

Charles Sheldon Hastings was born at Clinton, New York, on November 27, 1848. He was prepared for college in the Hartford High School, and graduated from the Sheffield Scientific School in 1870. He immediately became absorbed in certain astrophysical problems, and during the succeeding years published papers: On the comparison of the spectra of the limb and the center of the sun; On the comet Cruls; On the atmosphere of Venus in transit, and On the solar eclipse of July 29, 1878.

The years from 1874 to 1877 were spent in study abroad, during which time he was a student of both Helmholtz and Kirchhoff. On his return to this country, he was appointed associate professor of physics at the Johns Hopkins University, and began the intimate study of lenses, which occupied him all the rest of his life.

The ordinary formulas for the calculation of a lens system are extremely complicated, and the attempt to make them accurate enough to serve for the design-

ing of optical apparatus renders them almost unmanageable. Hastings conceived the idea that if he replaced the familiar ideas of indices of refraction, rays, and radii of curvature by the more natural notions of light velocities, wave and lens surfaces, which should be characterized by their curvatures, rather than by their radii, greater simplicity could be attained. He accordingly developed a method which in his hands gave very satisfactory results; by its use he was able to determine not only the spherical aberration and astigmatism of a lens, but the variation of its focal plane and its magnification, with variation of the wave-length of light.

So successful was he in these calculations, which required only the use of four-place logarithmic tables, that, as he said, he was able to do, rather simply, what elsewhere required a whole bureau of computers. The data for his computations were chiefly the indices of refraction of different kinds of glass. In order to carry out their measurement to the required degree of accuracy, he designed and constructed a spectrometer, of which the principal feature was a glass plate about 8 inches in diameter, which was divided at the limb into degrees, by diamond scratches. The settings were read by two micrometer microscopes of fairly high power, which were placed at the ends of a diameter.

The notable thing about the instrument was the great labor which he expended in determining the absolute position of each diameter. This instrument was his constant associate for over forty years, and enabled him to determine indices of refraction, accurately, to six decimal figures.

In the eighties, the variety of glasses then known did not permit the production of a truly achromatic combination. On account of the irrationality of dispersion there was always left an obnoxious secondary spectrum. With the passage of years, however, new kinds of glass were introduced, and Hastings by the study of the numerous specimens of glass was enabled by his calculations to combine them so as to practically rid objectives of secondary color. He was the first in this country to calculate their curvatures under the guidance of theory alone. But even then success was not to be hoped for by confining the effort to the mathematical aspect of the question. As the problem is a purely physical one, experiment could alone be used as an infallible guide. Mathematical consideration could only lead to the conclusion that certain constants which appear in the equations should have the same ratio as the dispersive ratio of the two varieties of glass which enter the construction. Unfortunately, the dispersive ratio is really indeterminate, in all practicable cases, and hence the necessity of testing the validity of any choice of a theoretical value for this ratio.

Any theory would adapt itself to a sufficiently small objective, and hence a rigid test could only be had by applying the theory to a large one. To have such a glass made greatly exceeded the means at his disposal, nor could he find a skilled optician who was able to shape the lenses to his curves. To meet this difficulty, he undertook to educate himself to perform this extremely accurate work. For this purpose he acquired a certain amount of general knowledge of the methods used by lens makers, by visiting the shops of various lens makers, both in this country, and in England, and in Germany, and finally, by placing himself for a time under the instruction of a skilled optician in Paris, he acquired the indispensable practical knowledge and dexterity which was essential for his purpose.

He now constructed a four-inch objective from two carefully studied glasses, on the hypothesis that the spherical aberration for rays of mean wave-length should vanish, and that the sum of the squares of the chromatic aberration for the other wave-lengths, weighted according to their respective luminosities, should be a minimum. The objective was carefully constructed, after improving the form of the spherometer so as to greatly increase its delicacy.

The experiment proved a failure. A careful remeasurement of the surfaces and a repetition of the computations demonstrated that the theory was certainly at fault.

He next selected a different method for determining the most appropriate value for the dispersive ratio, imposing a condition as to the amount of light energy sent very near the geometrical focal image of a distant point. A four-inch glass was ground in accordance with curves, computed on this new basis, and the telescope proved to be an admirable one, with corrections, both for spherical aberration and color, sensibly perfect. Successively, larger and larger objectives were later constructed, which proved of the highest excellence.

From 1884 till 1915, when he retired, Hastings was professor of physics in the Sheffield Scientific School, and for many years acted as consulting expert for the Brashear Company.

Obviously, an objective corrected for visual use is of no value for photographic purposes. Hastings, however, made correcting lenses for transforming visual into photographic refractors and brought the spectroscope to a very high degree of perfection.

The largest telescope of his design is the 30-inch photographic objective of the Allegheny Observatory, and is a very perfect glass. Other constructions from his calculations are the 26-inch photographic objective for the Yale Observatory in South Africa; the 24-inch visual refractor at the Chabot Observatory at Oak-

land, California; the 18-inch visual objective at the University of Pennsylvania; a twin doublet of 16-inch aperture at Heidelberg, Germany; a 72-inch reflector at Victoria, B. C., and the 15-inch photographic Loomis coelostat at the Yale Observatory, New Haven. This telescope has a focal length of fifty feet, and is the third longest telescope in the world.

Another of his achievements was the construction of an anastigmatic magnifier which is very widely used.

Two or three years before his death, he perfected an improvement in the microscope, which he regarded as highly successful and important, but his increasing disabilities prevented his communicating the detail of his design, and it is to be feared that his results are lost to the world.

Hastings was always interested in all optical phenomena. In 1920, he published a general theory of halos, on the hypothesis of two simple kinds of ice crystals, by which he was able to explain all the general features of complex halos.

His published works number some forty odd pamphlets, and the following three books: "General Physics," written in collaboration with Frederick E. Beach, in 1898; "Light," which was published in 1901, as one of the Yale Bicentennial Publications, and "New Methods in Geometrical Optics," published in 1927.

He was a man of wide culture, and deeply interested in literature, art and architecture. In 1909, after extended study of the notable asymmetries of the Cathedral of Pisa, and other European churches, he published a paper, "On the Architectural Refinements in Medieval Churches."

Hastings was always uniformly cordial but possessed a personal dignity through which one rarely passed to familiarity. He will always be remembered as a courtly gentleman of the old school.

FREDERICK E. BEACH

RECENT DEATHS

DR. LOUIS AGRICOLA BAUER, director emeritus of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington, died on April 12 as the result of a fall from a window of his apartment. He was sixty-seven years of age.

J. HAMMOND SMITH, of the College of Engineering of the University of Pittsburgh, died suddenly on April 13. He was sixty-five years old.

DR. DONALD R. DICKEY, research associate in vertebrate zoology at the California Institute of Technology, died on April 15, at the age of fifty-five years.

PROFESSOR CHARLES G. SIMPSON, professor of mathematics in the School of Engineering, Milwaukee, died on February 5, at the age of sixty years.

H. CHAPMAN JONES, formerly for many years senior demonstrator in chemistry in the Royal College of Science, London, died on March 7, aged seventy-seven years.

SIR HORACE PLUNKETT, first vice-president of the Department of Agriculture and Technical Instruction for Ireland, known for his contributions to Irish agriculture, died on March 26, aged seventy-seven years.

THE death is announced, at the age of seventy-eight years, of Sir Patrick Geddes, in Montpellier, France, where he was director of the Scots College. He was distinguished as a biologist and had held teaching posts in London, Aberdeen, St. Andrew's and Edinburgh universities.

SCIENTIFIC EVENTS

A NEW TUBERCULOSIS BUILDING AT THE PASTEUR INSTITUTE

To the Institut Pasteur de Paris has been added, according to the Paris Correspondent of the *Journal* of the American Medical Association, a large five-story building, erected in the gardens and reserved for studies on tuberculosis and for the preparation of the B C G vaccine, the demands for which have constantly increased, coming from all parts of the world. Only 890 doses were sent out in 1924 and in 1931, 101,646 doses. Since the creation of the B C G vaccine, 414,198 vaccinations have been performed. The new building cost $400,000, and was recently formally opened by the minister of public health, with the attendance of representatives of the medical press and numerous scientists. Mr. Calmette himself conducted visitors about the building. He showed the first strains of the B C G vaccine, which date from February 8, 1908. The transplantations of the cultures, from week to week, on ox bile were kept up until 1919, in order that the bacillus should become absolutely harmless, and it was not until five years later, experiments having been constantly made, in the meantime, on animals, and the demonstration having been made that the bacillus could not regain its virulence, that Calmette and Guérin decided to apply their vaccine to man. The recent Lübeck tragedy has clearly proved that only the introduction of virulent bacilli into the cultures can deprive them of their harmlessness. In the preparation of the vaccine, the most rigorous asepsis is followed. Every week transplantations of the cultures, which are preserved in baths at a temperature of 38° C., are made. The collected emulsion is distributed in tubes of 1 cc capacity, and

each containing 5 mg of tubercle bacilli. Each tube contains thirty doses, which, at the rate of three doses by mouth daily, suffice for ten days' treatment. One entire story of the building is reserved for these operations, this story being completely separated from the other parts of the building, in which are the laboratories of Messieurs Nêgre, Valtis and Boquet, the laboratories for the preparation of tuberculin, the anatomic laboratories, the laboratories for experimental physiology, chemistry and others. A *bacillothèque* (filing cabinet) contains all the strains of bacilli—human, bovine, avian and the like. The B C G strain is kept in a special bath, of which only Mr. Guérin has the key. In other cabinets are filed the records pertaining to each request for vaccine, together with the responses announcing the results secured, so that absolutely correct statistics are readily available. In the basement are the necropsy rooms and the cages for 6,000 animals (chiefly guinea-pigs) used for experiments. The Pasteur Institute uses 8,000 guinea-pigs annually.

THE X-RAY UNIT OF THE HOSPITAL OF THE UNIVERSITY OF MICHIGAN

AFTER four months of remodeling and installing new equipment, the University of Michigan Hospital, which now handles a volume of x-ray work second only to the Mayo Clinic in Minnesota, opened recently its new x-ray department, which is said to be unexcelled by any similar installation in the country.

The new unit is featured by a novel plan of rooms and apparatus which stresses convenience and privacy for patients and the quick processing of the x-ray films. From waiting rooms the patients are directed to private dressing rooms from which they pass as called by private corridors to the proper department. A complete "traffic system" of lights, which indicate what rooms are in use to all offices of physicians and the directing staff, makes possible quick routing of patients, so that routine x-ray examinations may be made at the rate of twenty an hour.

Rapid development of films to aid physicians to make an early diagnosis is made possible by a modern dark room, provided with dry air from which all moisture has been chilled out by special refrigerating machinery. Once in operation for the day, the room need never be lighted or work stopped, the completed films being passed out through a double-doored, light-tight well for final rinsing and drying From the drying rack they may be taken directly to the physicians' offices, each of which has its own viewing apparatus, so that a report may be given in a fraction of the usual time needed.

Among other details of the equipment are lead lined rooms which confine the rays of high voltage treatment apparatus, with lead glass windows through which the doctor or technician may observe the patient, a room in which x-ray films of the chest of bed-ridden patients may be taken through the bed by apparatus beneath the floor, special equipment for locating foreign bodies in the eye, and automatic apparatus, which, as soon as one patient's films are made, resets itself for the second stereoscopic exposing. Memorializing Dr. Preston M. Hickey, for many years head of the department, is a staff library which will contain publications on roentgenology, given by Dr. Hickey's friends, former associates and assistants, and a complete set of special x-ray studies of normal and diseased parts for reference. Classrooms, and research laboratories for medical students are also provided within the department. The memorial bas-relief of Dr. Hickey, presented this last year by the American Roentgen Ray Society, has been hung in the special conference room set aside for the members of the hospital staff who wish to review the examination of their patients with members of the roentgenology staff.

EXHIBIT OF THE PHILADELPHIA MINERALOGICAL SOCIETY

THE annual exhibit of the Philadelphia Mineralogical Society opened on April 17 in the Free Natural History Museum of the Academy of Natural Sciences. All the specimens are shown by members of a group of amateur mineralogists whose hobby is collecting minerals in near-by quarries, mines, valleys and hills, or wherever rocks are exposed.

The exhibit this year, which will be open free to the public until April 25, is confined entirely to minerals collected within a day's journey of Philadelphia in Pennsylvania and New Jersey—most of them rare and unusual crystals incidental to the mining and quarrying industry. For the granites, gneisses and other metamorphic rocks of the neighborhood frequently contain veins of interesting minerals. Over 100 minerals, or nearly ten per cent. of all those known to science, have been found within the city limits of Philadelphia.

Two groups of quartz crystals from Bridgeport, Pennsylvania, are exhibited by Mr. A. Fleming, Jr., and calcite crystals from Howellville, by Mr. E. H. Ceinkowski. Showy blue cyanite and garnet from Prospect Park, Delaware County, is exhibited by Mr. C. H. Jackson. A rather extensive series of local minerals is shown by Mr. Harry W. Trudell, director of the Frankford Institute for Medical Research, whose collection is perhaps the finest private one in Philadelphia.

The rock crystal and crystal ball are exhibited by Mr. Morrell G. Biernbaum. Petrified wood from Lindenwold, N. J., and Newton, in Bucks County, have

been loaned for the exhibit by Mr. W. H. Flack. Brilliant pyrite, magnetite, sphalerite and galena are mute evidence of the iron, lead and zinc mining near Phoenixville many years ago. A series of especial interest to beginners and boy scouts is that of "fifty minerals which can be found at 10 localities near Philadelphia." Other exhibitors are Messrs. J. S. Frankenfield, George Petersen, E. A. Groth, William Knabe and Horace J. Hallowell.

THE AMERICAN MEDICAL ASSOCIATION

THE annual meeting of the American Medical Association will be held at New Orleans, opening on Tuesday evening, May 10, when Dr. E. H. Cary, professor of ophthalmology and otolaryngology at the medical school of Baylor University and dean emeritus of the school, will be installed as president. He succeeds Dr. E. Starr Judd, of the Mayo Clinic, Rochester, Minnesota. The president's reception will be on Thursday evening, May 12.

The meetings of the scientific sections will begin on Wednesday morning, May 11, and will continue through Friday, May 13. These are as follows: Practice of Medicine and Diseases of Children; Surgery, General and Abdominal; and Obstetrics, Gynecology and Abdominal Surgery; Ophthalmology; and Laryngology, Otology and Rhinology; Pharmacology and Therapeutics; and Pathology and Physiology; Nervous and Mental Diseases; and Orthopedic Surgery; Preventive and Industrial Medicine and Public Health; and Gastro-Enterology and Proctology; Dermatology and Syphilology; and Urology; Radiology.

The House of Delegates will convene for its first meeting at 10 A. M. on Monday, May 9. All meetings of the House of Delegates will be held at the Roosevelt Hotel.

The clinical lecture program will be given on Monday afternoon, May 9, and on Tuesday morning and afternoon, May 10, at the Municipal Auditorium. Topics to be discussed are of important practical interest to the rank and file of the profession. This program will include the following lectures:

Drug Addiction—A World-Wide Problem, P. Wolff, Berlin, Germany.

Drug Addiction in the United States, Walter L. Treadway, Washington, D. C.

Cancer of the Gastro-Intestinal Tract, J. Shelton Horsley, Richmond.

The Changing Diet of the American People, Lafayette B. Mendel, New Haven.

The Diagnosis of Early Pulmonary Tuberculosis, Lawrason Brown, Saranac Lake.

Disturbances of Peripheral Circulation, Arthur W. Allen, Boston.

Appendicitis, H. A. Royster, Raleigh, N. C.

Early Diagnosis of Poliomyelitis, Lewis J. Pollock, Chicago.

Use of Convalescent Serum in Poliomyelitis, William H. Park, New York.

Orthopedic Treatment of Poliomyelitis, Willis C. Campbell, Memphis, Tennessee.

Motion Picture on Cancer, Francis Carter Wood, New York.

In addition to Dr. P. Wolff, secretary of the Narcotic Committee of the League of Nations, secretary of the German Council of Therapy and editor of the leading medical publication of Germany, who will participate in the clinical lecture program given above, Sir Arthur Newsholme, of Birmingham, England, known for his work in the field of public health, will be one of the participants in a joint symposium of the Section on Practice of Medicine and the Section on Preventive and Industrial Medicine and Public Health. Sir Arthur's topic will be "The Relation of Private Practice to Preventive Medicine in Europe." Professor N. Krasnogorski, of Leningrad, will contribute a paper on "Conditioned Reflexes in Psychopathology of Childhood" to the program of the Section on Diseases of Children.

FELLOWSHIP AWARDS IN THE SCIENCES AT YALE UNIVERSITY

THE Yale University Graduate School announces the award of 178 fellowships and scholarships, the recipients selected from more than one thousand applicants. Thirty-seven have been awarded research fellowships. As in former years these permit of an international exchange of scholars. From Göttingen will come Dr. Max F. Düring to work with Professor Oystein Ore on algebraic numbers, while Mr. Milton S. Plesset, of Pittsburgh, a candidate for the doctorate in physics at Yale this June, will go to Copenhagen to carry out further research under Professor Bohr. Two European zoologists, Dr. Paul A. Weiss, of Vienna, and Dr. Oscar E. Schotte, of Geneva, will continue research begun this year with Professor Ross G. Harrison in the Osborn Zoological Laboratory.

Dr. George Scheff, of Pecs, Hungary, has been offered a renewal of the Alexander Brown Coxe fellowship to continue his bacteriological research with Professor Leo Rettger, while Dr. Maximilian Steiner, of Vienna, has been awarded a Seessel fellowship to work with Professor George E. Nichols at the Osborn Botanical Laboratory on the chemical nature of changes in the osmotic pressure of plants.

Dimitri P. Krynine, a Russian engineer, known for his work in the field of soil mechanics, has been awarded a Sterling fellowship to pursue his research on the permeability of soils.

Ebbe C. Hoff, of Lindsborg, Kansas, a candidate for the Ph.D. at Oxford this year, has been awarded a

Sterling fellowship for research with Professor John F. Fulton in neuro-physiology.

For psychological research, Dr. Helen Peak, of Dallas, Texas, has been awarded a Sterling fellowship to enable her to continue work with Professor Raymond F. Dodge on the speed of reaction of the eyelid in organisms at various phylogenetic levels, while a somewhat related study of eye movements is made possible by the award of an Alexander Brown Coxe fellowship to Dr. Frank H. Couch, of Cromwell, Connecticut, to make it possible for him to carry on his work with Professor James C. Fox.

A special research fellowship in bacteriology has been awarded for the first time this year. Dr. Louis Weinstein, of Bridgeport, Connecticut, is the recipient. He will study the relationship of bacterial flora of the gastro-intestinal tract to the psychological reactions of the animal host.

A Sterling fellowship has been awarded to Charles E. Olmstead, of Roca, Nebraska, to study the effect of year to year differences in climatic conditions on wood structure of native trees. He will work with Professor Nichols at the Osborn Botanical Laboratory.

Paleontological research on the gastropods of the St. Louis, Pennsylvania outlier, has been made possible by the award of a Sterling fellowship to Dr. J. Brooks Knight, of Branford, Connecticut.

Mr. Frederick N. Rhines, of Toledo, Ohio, has been appointed Sterling fellow for research in metallurgy. He will study the rôle of oxygen in some of the common metals.

At the Sloane Physics Laboratory honorary research fellowships have been awarded to Dr. Malcolm Henderson and to Donald Cooksey, who is a candidate for the Ph.D. this June. Dr. Henderson will continue his experiments in the field of radioactivity while Mr. Cooksey works on precision measurements in high frequency spectra.

SCIENTIFIC NOTES AND NEWS

THE meeting of the National Academy of Sciences on the evening of April 25 will be given to the celebration of the hundredth anniversary of the electrical discoveries of Joseph Henry. The addresses will be: "Henry as a Physicist" by Dr. W. F. Magie, professor of physics at Princeton University; "Henry as an Electrical Pioneer" by Bancroft Gherardi, vice-president and chief engineer of the American Telephone and Telegraph Company, and "Henry as an Administrator" by Dr. C. G. Abbot, secretary of the Smithsonian Institution.

THE evening address of the American Philosophical Society will be given on April 23 by Dr. Paul van Dyke, professor of modern European history at Princeton University, who will speak on "The Human Washington." At the annual dinner on the evening of April 23 the speakers will be Dr. James Brown Scott, secretary of the Carnegie Endowment for International Peace; Dr. Howard McClenahan, secretary of the Franklin Institute; Dr. Thomas S. Gates, president of the University of Pennsylvania, and Dr. William B. Scott, emeritus professor of geology and paleontology, Princeton University.

THE annual meeting of the trustees of Science Service will be held in Washington on Thursday, April 28. On the preceding day there will be a round table conference and a dinner to consider how Science Service may further its purpose of making science better known and appreciated by the general public and contribute its share in advancing modern civilization. Those who will take part in the conference include Dr. Karl T. Compton, Massachusetts Institute of Technology; Dr. Edwin G. Conklin, Princeton University; Dr. Simon Flexner, Rockefeller Institute for Medical Research; Dr. Frank B. Jewett, Bell Telephone Laboratories; Mr. A. H. Kirchhofer, the *Buffalo Evening News;* Dr. John C. Merriam, Carnegie Institution of Washington; Dr. A. A. Noyes, California Institute of Technology; Mr. Robert P. Scripps, Scripps-Howard Newspapers; Dr. E. B. Wilson, Harvard University School of Public Health. The speakers at the dinner will be Dr. John H. Finley, *The New York Times;* Dr. Robert A. Millikan, California Institute of Technology, and Dr. H. G. Moulton, Brookings Institution.

DR. ARTHUR P. COLEMAN, emeritus professor of geology, University of Toronto, has been awarded the Victoria Medal of the Royal Geographical Society in recognition of his geographical work in Canada. Professor Coleman celebrated his eightieth birthday on April 4, when he was the recipient of many messages of congratulation from friends and colleagues at home and abroad. He is actively engaged in preparing a report on the glacial geology of southern Ontario for the Ontario Department of Mines.

DR. SELMAN A. WAKSMAN, of the department of soil microbiology, Rutgers University, and the New Jersey Agricultural Experiment Station, has been elected a member of the Imperial German Academy of Sciences at Halle, in recognition of his work on the microbial population of the soil.

PROFESSOR ROGER L. MORRISON, of the highway engineering department of the University of Michigan, has been elected an honorary member of the Institute of Highway Engineers at Cambridge, England.

IT is reported in *Nature* that in view of the conferring of the honorary degree of D.Sc. by the University of the Witwatersrand, Johannesburg, on Mr. N. E. Brown, in recognition of his work on the South African flora, Sir Arthur Hill presented Dr. Brown to his old colleagues in the Herbarium of the Royal Botanic Gardens, Kew, on March 19. In the course of a short address, Sir Arthur said: "Dr. Brown was on our staff for forty-one years, from 1873 until 1914. He was elected an associate of the Linnean Society in 1879, and in 1921 he received the Senior Captain Scott Medal for scientific research in South Africa from the South African Biological Society. I need not detail his many contributions to South African botany, as they are so well known to all botanists, but it is interesting to record that the following eminent South African botanists warmly supported the proposal that Dr. Brown should be honored by South Africa: General J. C. Smuts, Dr. I. B. Pole Evans, Professor J. W. Bews, Professor R. S. Adamson, Professor R. H. Compton and Dr. John Muir."

DR. FREDERIC H. LAHEE, chief geologist of the Sun Oil Company, Dallas, Texas, who has been for three years third vice-president of the American Association of Petroleum Geologists, was elected president at the annual meeting.

AT the ninth annual meeting of the Alabama Academy of Science, held at Howard College, Birmingham, on March 11 and 12, the following officers were elected: *President,* Dr. J. F. Duggar; *Vice-presidents,* Dr. J. L. Brakefield and Dr. H. C. Heath; *Secretary,* Dr. B. P. Kaufmann; *Treasurer,* Mr. A. G. Overton; *Editor,* Dr. E. V. Jones; *Councillor to the American Association for the Advancement of Science,* Dr. E. B. Carmichael. Birmingham-Southern College was selected for the 1933 meetings.

AT the annual business meeting of the Royal Philosophical Society of Glasgow, held on March 23, the following officers were elected: *Vice-president,* Dr. Henry L. G. Leask; *Honorary Librarian,* Dr. James Knight; *Honorary Treasurer,* Sir John Mann; *Secretary,* Mr. John A. Buyers; *Members of Council,* Mr. John P. Heslin, Miss M. A. Hannan Watson and Mr. George B. Wishart.

PROFESSOR FRANK A. WAUGH, who for the past thirty years has directed the horticultural work of the Massachusetts State College, will retire on September 1 as head of the horticultural division in order that he may devote his entire time to the department of landscape architecture, of which he will continue as head. Dr. Ralph A. Van Meter, professor of pomology and a member of the staff for fifteen years, was promoted to head of the division. Professor Waugh is now on a six-months' leave in Japan for the purpose of studying Oriental gardening methods.

MR. ROBERT L. NUGENT, until recently physical chemist at the Gladwyne Research Laboratory and instructor in biochemistry in the Graduate School of Medicine of the University of Pennsylvania, has become assistant professor of chemistry at the University of Arizona.

DR. EDWIN O. JORDAN, of the University of Chicago, has been appointed a member of the Chicago Board of Health.

THE first Joseph A. Capps Prize for medical research of the Institute of Medicine of Chicago has been awarded to Dr. Warren B. Matthews, assistant resident in surgery at the University of Chicago clinics, for his paper on "Studies on the Etiology of Gastric and Duodenal Ulcer." This prize of five hundred dollars, established through the generosity of an anonymous donor, is awarded in alternate years for the most meritorious medical research by a graduate of a medical school in Chicago completed within two years after graduation.

THE award of the J. T. Baker Chemical Company Eastern Analytical Fellowship for 1932–33 has been made to Mr. Eugene Wainer, a graduate student at Cornell University. Mr. Wainer has worked with Professor Papish in research for Element No. 87 (*Journal of American Chemical Society,* 53, 3818, 1931). He plans to make a study of x-ray spectroscopy as a means of quantitative analysis under the direction of Professor Papish.

IN a letter written at Iquitos, Peru, on March 18, M. W. Stirling, chief of the Bureau of American Ethnology, states to the Smithsonian Institution that he had just completed an 800-mile trip on a raft via the rivers Yangtse, Santiago, Maranon and Amazon. His ethnological collection of Jivaro and Aquaruna material came through in good shape. Mr. Stirling hopes to reach Washington early in May.

DR. MARGUERITE LEFEVRE, chef de travaux in the University of Louvain, who as the holder of a fellowship under the Commission for Relief in Belgium Educational Foundation, has been prosecuting studies in geomorphology at Columbia University, is devoting a month to the study of ancient marine levels in South Carolina, Georgia and Florida.

DR. CHARLES E. SPEARMAN, of the University of London, will deliver on April 23 the principal address at the annual meeting of the University of Kentucky chapter of Phi Beta Kappa. His subject is "The Abilities of Man."

THE annual lecture of the James A. Gibson Ana-

tomical Society at the University of Buffalo was given on April 8 by Dr. G. Carl Huber, professor of anatomy and dean of the Graduate School at the University of Michigan. The subject of Dr. Huber's address was "The Kidney."

DEAN GEORGE F. KAY, of the State University of Iowa, gave the address at the annual Phi Beta Kappa, Sigma Xi Convocation at Washington University, Saint Louis, on April 7. His subject was "Scientific Discovery and Human Outlook." While at the university, Dean Kay also gave a lecture before the faculty and students of the science departments on "The Glacial History of Iowa and Adjacent States."

DR. R. A. EMERSON, head of the department of plant breeding in the New York State College of Agriculture, Cornell University, gave the third series of annual lectures under the Frank Azor Spragg Memorial Fund, from April 5 to 8, at Michigan State College. This memorial is in honor of Professor F. A. Spragg, who was in charge of plant breeding work at the Michigan Agricultural Experiment Station from 1906 until his death in 1924. The subjects dealt with in the lectures were: "Cooperation in Plant Breeding"; "Heredity and Environment" (a non-technical lecture); "Breeding White Beans for Disease Resistance, Yield and Canning Qualities"; "The Present Status of Corn Genetics and Cytology," and "The Solving of a Genetics Problem—the Inheritance of Variegated Pericarp in Corn."

Two lectures will be given on May 3 and 5 under the Edward K. Dunham Lectureship for the Promotion of the Medical Sciences at the Harvard Medical School by Dr. Ludwig Pick, professor of pathology, University of Berlin, and director of the Pathological-anatomical Institute Municipal Hospital, Friedrichshain, Berlin. The lectures will be given each day at 5 o'clock. The subjects will be " A Classification of the Diseases of Lipoid Metabolism and Gaucher's Disease" and "Niemans-Pick's Disease and other Forms of so-called Xanthomatosis."

SIGMA PI SIGMA, honorary physics fraternity, installed its twenty-first chapter at the University of Richmond on March 16. The installation ceremonies were held in the physics lecture room of Richmond Hall with Dr. Marsh W. White, the executive secretary of the society, and Dr. R. C. Young, former president and member of the executive council, as installing officers. Nineteen charter members, including Dr. R. E. Loving, head of the physics department, and Dr. C. L. Albright, were initiated as charter members of the Chi chapter. After the installation dinner the first open meeting of the chapter was addressed by Dr. White, who spoke on "Energy Relations in X-ray Tubes."

THE thirteenth annual meeting of the American Geophysical Union will be held in the building of the National Academy of Sciences, Washington, on April 28 and 29. Dr. William Bowie is chairman of the union. Dr. L. H. Adams is vice-president and Dr. J. A. Fleming general secretary. The chairmen of the sections are: Geodesy, W. D. Lambert; seismology, N. H. Heck; meteorology, G. W. Littlehales; terrestrial magnetism and electricity, D. L. Hazard; oceanography, A. H. Clark; volcanology, A. L. Day, and hydrology, O. E. Meinzer.

THE second International Congress of Tropical Medicine, which was to have been held in Amsterdam next September, has been postponed until financial conditions are more favorable.

DEFINITE plans have been made for the removal of the North East Forest Experiment Station from Amherst to New Haven. Three specialists moving with the station are Dr. Perley Spaulding, forest pathologist of the bureau of plant pathology; Harvey MacAloney, entomologist of the bureau of plant entomology, and Paul Miller, of the bureau of biology.

THE Eugene Littauer Memorial Laboratory for diagnosis and research was recently presented to the Nathan Littauer Hospital in Gloversville, N. Y., by Lucius N. Littauer. The laboratory is a memorial to Mr. Littauer's brother and the hospital, built several years ago, to his father. Among guests at the dedication were Drs. James Ewing and Emanuel Libman, New York; Elliott P. Joslin, Boston; Edward Francis, Washington, D. C.; Milton C. Winternitz, New Haven, and Harrison S. Martland, Newark, N. J., all of whom made addresses. The building has two floors; on the first are the diagnostic laboratories and the second is planned for research, while the basement contains the necropsy, histologic and photographic rooms.

Nature reports that Lord Wakefield has given a sum of £25,000, spread over seven years, as a contribution to the British Imperial Institute. The institute is financed by annual grants from the Treasury, the Dominions, India and the Colonies, and was faced with the prospect of curtailment of its activities, owing to temporary reduction of some of its supporting grants. It is hoped that Lord Wakefield's gift will enable the institute to tide over the period of difficulty.

Museum News reports that a museum is included in the plans for the building of the Institute of Polar Research to be erected at Cambridge, England. The building is to be a memorial to Captain Robert Falcon Scott, whose navigation book for his last expedition will be one of the exhibits. Specimens of sledging gear, clothing, cooker's instruments and Polar equipment of all kinds will be collected. Material from

Sir John Franklin's expedition will be included. A grant of $200,000 has been contributed toward the building by the Pilgrim Trust Fund, founded by Mr. Edward S. Harkness.

THE Munich correspondent of *The New York Times* reports that another German expedition is being made to the Himalaya this summer. The party, which will be nine or ten in number, consists for the most part of first-class Bavarian mountaineers and members of the Munich Alpenverein, but two or three members of the American Mountaineering Club are on their way from the United States to take part in the expedition. The expedition will attempt the ascent of Nanga Parbat, 26,629 feet, in the Kashmir Himalaya. If successful, the climbers will have reached a summit higher than any yet attained by man, though not the highest point reached—a record held by Mallory and Irvine on Mount Everest. Only one previous attempt has been made to scale Nanga Parbat; this was in 1895, when the British mountaineer, A. F. Mummery, lost his life in the venture. The leader of the German expedition is Herr Wilhelm Merkl, a Munich engineer, who, in 1929, took part in an expedition across the Caucasus which included an ascent (the third on record) of the extremely difficult Uschba Mountain. The expedition will leave Munich on April 26 and will depart from Srinagar, the capital of Kashmir, *en route* for the mountain, early in June.

DISCUSSION

THE ANALYSIS OF ELECTROMYOGRAMS

THE method described by Travis and Hunter[1] in a recent issue of SCIENCE for the study of the human electromyogram calls for comment, since it seems to be almost expressly designed to obscure well-established physiological facts concerning the activity of the central nervous system. Their method consists essentially of determining what percentage of the "output voltage" will pass various electrical filters designed to eliminate alternating current components above a given frequency. The authors offer the method as a means of "reading" the irregular electrical disturbances generated during muscular contraction. The implication is inescapable that they anticipate analyzing the "action current wave" into its components and interpreting variations which may be found under various conditions. They neglect, however, to evaluate their method in the light of the known facts of muscular physiology. The action currents of muscles in voluntary contraction present an irregular and confused picture due to the combined asynchronous activity of many individual units. Each active unit generates a series of electrical pulses or action currents at a rather slow and more or less regular rhythm. The way to clear interpretation does not lie in an analysis of the resulting total disturbance in terms of frequencies as revealed by electrical filters, since this method implicitly assumes that the whole is composed of a variety of alternating current components of various frequencies. The total action current can be described in this way, but the result has little connection with the underlying phenomena, and therefore can only be misleading.

Adrian and Bronk[2] have shown that by means of a hypodermic needle, which contains within it a fine insulated wire, effective contact can be made with a relatively small number of active muscular units. We have repeated their observations and confirmed the fact that the rhythms of individual units appear clearly in the record of contractions of moderate intensity. This method offers a direct analysis of the situation, and demonstrates immediately that the number of impulses per second in each motor unit in voluntary contractions is of the order of ten to fifty or a hundred. The method of Travis and Hunter completely misses this fundamental fact, only 10 per cent. of the total action current being ascribed by them to frequencies below one hundred.

The peak of frequencies represented, according to Travis and Hunter, lies at about 400 per second. This result almost certainly depends upon the time-relations of the individual action currents in the muscle fibers. These rise from zero to their maximum voltage in approximately a thousandth of a second and fall again somewhat more slowly. Waves of these time-relations, whether recurring regularly or irregularly, when confronted by a series of graded electrical filters will not be greatly reduced by those which pass frequencies up to 1,000 per second but will be considerably reduced by those whose limit is 400 and very much so by 200. In other words, these waves will have a large component between 300 and 500 when analyzed by filters, whether the waves occur singly, or at regular intervals or at random. Travis and Hunter's frequency curve therefore depends primarily upon the shape of the individual impulses and not upon the fequency of their occurrence. It is the latter which varies with different degrees of activity of the neuro-muscular system while the former is relatively constant and depends upon the local condition of the muscle itself. If it is desired to study frequency it is now quite easy to obtain a record of a series of indi-

[1] L. E. Travis and T. A. Hunter, SCIENCE, February 19, 1932.

[2] E. D. Adrian and D. W. Bronk, *J. Physiol.*, Vol. lxvii, No. 2, p. 119, 1929.

dual impulses which one may "read" quite easily, d, if desired, a Fourier analysis may be applied to e oscillograms of the individual impulses.

Why endeavor to infer the properties of an indidual egg from an omelette, even by means of elabrate chemical analysis, when the individual eggs can e purchased separately?

H. DAVIS
A. FORBES
L. GARCEAU

THE PINE FORESTS OF COSTA RICA

IT was not through oversight that in his "Plantas Usuales de Costa Rica" Professor H. Pittier failed to mention the occurrence in that country of pine trees. No one knows the vegetation of Costa Rica so well as the writer of that now almost classic report, and if trees so important economically as pines did occur in Costa Rica, he could not be pardoned for neglecting them.

But, in a recent volume devoted to the "Tropical Forests of the Caribbean" (Gill, 1931), the reader is informed that "In British Honduras pine extends practically to the coast, but in Costa Rica it is confined to the uplands, and in Panama fades out entirely" (whatever the last phrase may mean). Opposite the page (p. 151) on which this statement appears, there is a map illustrating the distribution of pine forest in Central America. There is a modest strip of black extending the length of what apparently is intended as the main cordillera of Costa Rica (which does not exist), with a discreetly reduced, isolated patch of black farther south, near the Panama border.

Further, on page 317, under Table No. 3, it is stated that the forest area of Costa Rica consists of eight millions of acres, of which one million acres are reported optimistically as "conifers." Under "remarks" there appears the distressing information, "Pine type badly injured by fire."

The present writer is not prepared to criticize the forest data reported for other countries, although to one who has traveled by railroad from Puerto Barrios, Guatemala, to Guatemala City it is evident that the distribution of pine forest as indicated for that country is erroneous. Likewise, a botanist who has studied the flora of the Pacific coast of Central America will be unable to comprehend why Pacific Nicaragua should be mapped as "deciduous" forest, and all the rest of the Central American coast as "rain" forest. The facts are that exactly the same species of trees characterize the Pacific flora all the way from Guatemala at least as far as the Nicoya Peninsula in Costa Rica, and to Panama, for that matter. Floristic areas, unfortunately, do not usually coincide with political boundaries; it would make work easier for the systematic botanist if they did.

The subject of the southern limit of pine trees (and of all conifers) in North America is a matter of substantial scientific and economic interest, else it would not deserve mention here. In a book which purports to be scientific, the information published should be as accurate as possible, or else omitted. The distribution of pines in tropical America has been discussed by competent writers, and the facts could have been obtained without any great amount of research. Thomas Belt, in "A Naturalist in Nicaragua," reports that when traveling northward from Chontales, he observed the first pines in the vicinity of Matagalpa, and that is probably their southernmost extension in North America. (They do not occur, of course, in South America.)

The quotation cited in the second paragraph of this article should be corrected to read: "In Costa Rica pines are confined to dooryards," and in the table mentioned the area should be reduced from one million acres to one acre. A plot of that size, perhaps with a little crowding, would contain almost all the pine trees of Costa Rica. They do exist, it is true, for in two winters passed in the "uplands" I have seen some of them, but they were always about houses, and evidently planted. Systematic questioning of country people and of educated persons thoroughly familiar with the country failed to elicit a single report of the existence in Costa Rica of a native pine tree. In a recent letter to the present writer, Mr. F. Charles Clark, a lumber exporter of San José, Costa Rica, writes: "I have traveled practically all over the forests of this country, I might say, looking for native pine (genus *Pinus*), without success. I can not imagine anyone making a statement that there are millions of acres of pine trees in this country, and I, for one, should like to come across the first native tree." Friends of forest conservation may be comforted by the fact that under the circumstances it is scarcely possible that the Costa Rican pines have been "badly injured by fire."

PAUL C. STANDLEY
FIELD MUSEUM OF NATURAL HISTORY

THE SOARING OF TURKEY BUZZARDS

THOUGH the means by which certain birds accomplish their soaring flight with wings held stationary is by no means the mystery it was before man learned to make use of upward air currents in flight with motorless gliders, the following observation seems of interest. The observation may not be unusual, yet neither myself nor any of the half dozen others who witnessed it had previously noticed it.

On a calm, very hot afternoon in August, 1931, in the level region of east central Illinois, a straggling flock of turkey buzzards were observed approaching from the north at a height of approximately five hundred feet. They were soaring along on a straight course with wings stationary. Not far from the observers they came underneath one of the scattered cumulus clouds present at the time and at once discontinued their flight toward the south and began to soar in circles, keeping directly beneath the cloud. During this circling, with wings still held stationary, they rapidly gained elevation, so that the course of each bird became a spiral. This upward, spiral, soaring flight continued for some minutes until the birds had become hardly visible, when they again resumed their southward course at a much greater elevation than before they had come underneath the cloud. A few minutes later a second flock of about a dozen came along from the north and under the same cloud gained elevation, as had the first one.

This is evidently a case where the birds made use of the upward air current beneath the cumulus cloud to gain elevation without resorting to wing strokes and the writer has been wondering if this is matter of common observation.

G. S. DODDS

WEST VIRGINIA UNIVERSITY

ON THE EARLY HISTORY OF PLATINUM

IT is quoted generally throughout text-books and related literature that the first mention of platinum occurs about 1750. James Lewis Howe, in his "Bibliography of the Metals of the Platinum Group," including the years 1748 to 1917, states that the first reference to platinum is in the volume of don Antonio de Ulloa, "Relacion historica del viaje a la America Meridional," published in Madrid, 1748. The next reference is of 1751 when platinum was described in the literature by Watson and Brownrigg, and in the same year the properties of the ore were described by T. Scheffer.

As we know from historical record, don Antonio de Ulloa was not the first to write of the New Americas nor even among the very first to go there from the old country, so that it would not be altogether amiss to suppose that earlier commentators on the new lands, especially on Nuevo Granada as the Republic of Colombia was then known, might make mention of this curious "unripened gold"—as the Indians indigenous to those regions where it was found believed it to be and had planted it back in the river bed to give it more time to ripen into the yellow metal gold. In the Archivo General de Indias in Seville and in the Archivo Historico and Biblioteca Nacional in Madrid, Spain, there are kept the records of the transactions, descriptions and observations in minute detail between these extraordinary colonies and their mother country. There are, in addition, other libraries and private collections in Spain pertaining to the Americas, but it was among the three named that I was engaged upon another problem when I found references to platinum dated 1735. Some of these preserved documents refer to the shipment of rather large quantities (in one case 18 pounds) of alloyed metal which was called platina. In one document mention was made of a refined platinum, which had been treated with mercury prior to its shipment to Spain.

This (1735) is twelve years before don Antonio de Ulloa set out from Spain to make his scientific voyage (to measure meridian) and subsequently write his "Relacion Historica," and I have reason to believe that earlier references exist among this literature.

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REPORTS

THE INCORPORATION OF SCIENTIFIC SOCIETIES

AT the Cleveland 1930–31 meeting of the American Society of Plant Physiologists the question as to the advisability of incorporating the society was referred to a committee.

Of the twenty-one scientific societies investigated, fifteen are incorporated. Seven of these societies are incorporated in the District of Columbia, one in Illinois, one in Wisconsin, one in Massachusetts, one in New York and one in Pennsylvania. As the reasons for and ways of incorporating learned organizations may be of interest to other scientific societies the findings of the above committee are here summarized.

The advantages of incorporation are important, inasmuch as it establishes a learned non-profit organization as a legal entity, thus bestowing:

(1) Freedom from financial responsibility in any lawsuit against the members on account of any action of the society.

(2) The ability to hold property and to receive gifts and bequests.

A few scientific societies reported to our committee that incorporation had resulted in the accumulation of funds for research, etc., by giving greater security to endowment funds and thus making it easier for donors to give relatively large gifts and bequests.

The possible disadvantages are related to the freedom of action of the organization. An unincorporated

society may do as it chooses, whereas upon incorporation the organization must comply with the laws of the jurisdiction where it is incorporated. Practically, however, the disadvantages resulting from this restraint or freedom of action is regarded by legal authorities as being slight. The statutes of some jurisdictions impose fewer restraints than others and the requirements for headquarters, annual meetings, reports, etc., differ in different jurisdictions.

The procedure of incorporating under the laws of the District of Columbia, and also in certain other jurisdictions, is relatively simple. The statutes of the District of Columbia relating to corporations and of certain other jurisdictions relating to learned *non-profit* organizations neither require a resident director nor the maintenance of an office in the jurisdiction. Meetings may be held anywhere and at any time without restriction.

The principal requirements under the laws of the District of Columbia may be taken as an example of the general procedure: (1) Any three or more persons of full age, citizens of the United States, the majority of whom are citizens of the District of Columbia may incorporate; (2) a certificate in writing must be filed in the office of the Recorder of Deeds stating (a) the name of the society, (b) the term for which it is incorporated, which may be perpetual, (c) the business and objects of the society, (d) the number of its trustees, directors or managers for the first year of the incorporated society's existence.

Upon the execution of the articles of incorporation of the society and the deposit of same with the recorder of deeds, the incorporators then form a temporary organization in Washington, D. C., at which meeting one of their number is elected temporary chairman and another temporary secretary. The constitution and by-laws of the organization as then standing are adopted, and all the then existing members of the society are declared elected members of the corporation with their present rights and privileges. The meeting then adjourns to assemble at the next regular place of meeting of the now incorporated society.

Many of the scientific societies investigated dispensed with legal assistance. Under such conditions the only expense involved is the registration fee of two or three dollars. A few societies, however, obtained advice of counsel in making such changes in their by-laws to conform with the statute under which incorporation is sought. The expense in such cases is about one hundred dollars.

A comprehensive discussion of the business relations of non-profit corporations is given by Harriman in Corporate Practice, Review 1, 7–11, 1929.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

APPARATUS FOR DETERMINATION OF CO_2 AND O_2 OF RESPIRATION

IN determining the respiration rate of fruit in storage a modification of the apparatus described by Magness and Diehl[1] has been used. The apparatus as modified has proved very satisfactory and is described here in the hope that it may be useful to others interested in respiration rate determinations.

With this apparatus the O_2 consumed as well as the CO_2 evolved can be measured. It consists (Fig. 1) of a water reservoir "A" which is connected by a siphon to a beaker "B" in which the water is maintained at a constant level by means of the siphon feed. The beaker "B" is connected by tubing to a liter-graduated cylinder "C" containing oxygen. The cylinder "C" is connected by capillary tubing to the respiratory chamber "D," which consists of a desiccator with a funnel fitted into the tubulature of the lid and the stem extending down to a second funnel which is fitted into a hole bored through the bottom of the desiccator. The apparatus of Magness and Diehl is modified in that the container for the O_2 is a graduated cylinder instead of a bottle and the beaker for the KOH solution is replaced by the two funnels in the

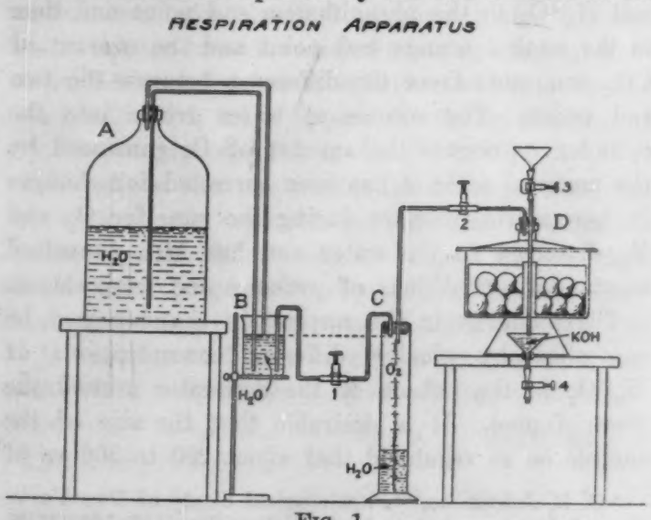

FIG. 1

[1] J. R. Magness and H. C. Diehl, "Physiological Studies on Apples in Storage," *Jour. Agr. Res.*, 27: 1–38, No. 1, 1924.

desiccator. The use of a graduated cylinder for the O_2 has been previously described by Johnstone[2], but was developed independently by the authors. These changes greatly facilitate the measurement of the O_2 and the changing of the KOH solution and make possible more accurate determinations.

To determine the respiration rate the material to be studied is weighed into a round wire mesh basket that fits into the desiccator. This basket has a tubulature through the center so that when it is placed in the desiccator the stem of the upper funnel can extend down to the lower funnel. After sealing the lid on the desiccator, the upper funnel is fitted into the lid and a solution of twice normal KOH (usually 25 ml) is drained into the lower funnel through the upper funnel and rinsed down with a small quantity (10 ml) of water. The O_2 cylinder is connected to the beaker "B" and the clamp at "1" opened. The desiccator is then connected to the O_2 cylinder and the clamp at "2" opened. The lid must be sealed airtight to the desiccator and all connections must be airtight.

As the material respires, CO_2 is given off and O_2 absorbed. The CO_2 given off is absorbed by the KOH solution in the lower funnel and the O_2 used is replaced by O_2 from the cylinder C. The O_2 withdrawn from C is replaced by water from B. Thus the system remains at atmospheric pressure and the concentration of O_2 and CO_2 in the atmosphere in the respiration chamber remains practically unchanged.

At the end of a run the clamp at "2" is closed and the KOH solution in the lower funnel drained into a flask. The lower funnel is then rinsed into the flask by adding water through the upper funnel. Fresh KOH is added through the upper funnel, the amount of water in the cylinder recorded, the clamp at "2" reopened and the respiration determined continued. The KOH is titrated against standard 2 normal H_2SO_4 to the phenolthalein end point and then to the methyl orange end point and the amount of CO_2 computed from the difference between the two end points. The volume of water drawn into the cylinder represents the amount of O_2 consumed by the material after it has been corrected for changes in barometric pressure during the run, for O_2 and N_2 dissolved in the water and has been converted to standard conditions of pressure and temperature.

The humidity in the respiration chamber may be controlled by placing different concentrations of H_2SO_4 in the bottom of the desiccator around the lower funnel. It is desirable that the size of the sample be so regulated that about 200 to 300 cc of O_2 are used and about a third of the KOH neutralized by the CO_2 evolved during a run. When this is done there is no appreciable accumulation of CO_2 in the respiratory chambers. If about half or more of the KOH is neutralized the phenolthalein end point in the titration becomes indistinct and there is likely to be an accumulation of CO_2 in the respiratory chambers due to the decreased efficiency of the KOH solution. On the other hand, if less than 100 cc of O_2 are used during a 24 hour period, a lowering of the atmospheric pressure of more than .3 inch may cause the gas in the desiccator to expand more than 100 cc and result in a flow of gas from the respiration chamber to the oxygen cylinder.

[2] G. R. Johnstone, "Physiological Study of Two Varieties of *Ipomoea batata*," *Bot. Gaz.*, 80: 145–167, 1925.

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VEGETABLE PEPTONE AGARS FOR QUANTITATIVE WORK WITH LACTOBACILLUS ACIDOPHILUS[1]

LACTOBACILLUS ACIDOPHILUS does not grow well on plain nutrient agar. To meet this difficulty, various special media have been suggested from time to time. The most widely used of these are: the whey agar proposed by Rettger and Cheplin,[2] 1912; the galactose agar of Rettger and Kulp,[3] 1922; the casein digest galactose agar of Kulp and Rettger,[4] 1924; the whey peptone galactose agar of Kulp,[5] 1926, and the tomato peptone agar of Kulp,[6] 1927. The third and fourth named are made in the dehydrated form by the Difco Laboratories, Detroit, Mich.

In connection with work on another problem in which we used acidophilus milks made with the "Scav" strain, we amassed considerable quantitative data consisting of a series of parallel counts on galactose whey agar and a cabbage agar which proved very satisfactory.

[1] Read at the Baltimore meetings of the Society of American Bacteriologists, Dec., 1931.

[2] L. F. Rettger and H. A. Cheplin. "The Transformation of the Intestinal Flora with Special Reference to the Implantation of *Bacillus acidophilus*," Yale University Press, 1921.

[3] L. F. Rettger and Walter L. Kulp, "A Note on the Choice of Culture Media for the Study of Lactobacillus with Special Reference to the Carbohydrate Employed," *Abs. Bact.* 6, 24, 1922.

[4] W. L. Kulp and L. F. Rettger, "A Comparative Study of *L. acidophilus* and *L. bulgaricus*," *Jr. Bact.*, 9, 357–394, 1924.

[5] W. L. Kulp, "The Determination of Viable *Lactobacillus acidophilus*," SCIENCE, 64, 304–306, 1926.

[6] Walter L. Kulp, "An Agar Medium for Plating *L. acidophilus* and *L. bulgaricus*," SCIENCE, 66, 512–513, 1927.

The cabbage agar was prepared in liter quantities by shredding 250 grams of fresh cabbage, adding one liter of tap water, and boiling for one half hour. The decoction was then decanted off, the loss of water restored, and the liquid filtered through filter paper. To this liquid, ten grams of peptone (Difco), five grams of sodium chloride, two and one half grams of ortho sodium phosphate, and ten grams of Difco agar were added. The mixture was heated in the steamer until the ingredients were in solution. A very clear medium was obtained by sedimentation and removal of detritus by decantation of the clear liquid. This medium was sterilized in the autoclave at fifteen pounds pressure for fifteen minutes. The reaction varied somewhat but the pH was usually about 6.4 which is within the range for the good growth of the Lactobacilli, so that we have not regarded it necessary to adjust the reaction. A few experiments indicate that the sodium orthophosphate may be omitted from the formula without decreasing the value of the medium, and in some other experiments both the phosphate and the peptone have been omitted without decreasing its value as a culture medium.

In the process of plating, the medium was poured directly from bottles or flasks into the Petri dishes, about 16 or 20 cc per plate. When the agar had hardened, the plates were inverted and placed in closed containers and surrounded with an atmosphere of approximately ten per cent. carbon dioxide. The plates were incubated at 40° C. for 48 hours. The excessive amount of water of condensation was absorbed by the use of clay tops or by fitting disks of blotting paper into the tops of the Petri dishes.

A simple microscope, magnifying three and one half times, was used in counting.

The type of colony which appears on cabbage agar shows the rough irregular edge which has been considered characteristic of *L. acidophilus*. On the galactose whey agar there is a tendency for the growth to be compact, while the cabbage agar shows a thinner, somewhat more transparent, and more spreading colony. Although the colonies on the cabbage agar are more transparent, they are easily seen with the naked eye because of their larger size. The first fifty colonies taken at random on each of two plates, which were exact duplicates except for the medium, were measured with an ocular micrometer. The fifty colonies on the whey agar averaged 396 microns in diameter, while the same number on the cabbage agar averaged 531 microns.

Our tabulated quantitative data shows very plainly two interesting trends: first that the cabbage agar counts are higher than the galactose whey agar counts with consistent regularity, and second, that the higher counts on the cabbage agar are especially pronounced on the milks that are from two to three weeks old. Thus in a group of forty-five counts on milks which were from one to seven days old, the ratio of the galactose whey count to the cabbage agar count was as 1:1.55; on milks eight to fourteen days old, the ratio was 1:2.94; on milks sixteen to twenty-one days old, the ratio was 1:2.28, and on milks twenty-two to twenty-eight days old, the ratio was 1:2.17. This may be taken to indicate, it seems to us, that there are substances in the cabbage which stimulate the old bacterial cells in the milk to a vigorous growth, while such substances are lacking in the galactose whey agar. This would seem to be a desirable quality for a medium to possess if it is to be used for controlling a commercial product since it seems fair to assume that similar stimulating substances may exist in the intestinal tract of man. At any rate, the results indicate that many bacteria which are supposedly dead when their viability is tested by seeding in galactose whey agar, are alive and vigorous in our cabbage agar.

The method described for making cabbage agar was used in the preparation of other vegetable agars. Carrots and green beans were used in the same proportions as the cabbage. In tomato and spinach the proportion of the vegetable was doubled. Growth on green bean and spinach agars was very good, yet our data seem to indicate that these two vegetables are slightly less favorable than cabbage. Carrot and tomato agars, on the other hand, gave results very similar to those obtained with cabbage.

Since some milk product has been so generally used in the various media recommended for the growth of *L. acidophilus*, and since we found extracts from various vegetables to be very favorable, it occurred to us that a combination of such materials might be still more favorable. With this in mind, a tryptic casein digest prepared according to the method of Kulp and Rettger,[4] was added to our cabbage agar. With this medium we have obtained on some plates colonies which average twice the size of those on our vegetable media, but in sharpness of definition and in quantitative results, such media appear to offer no advantages over the simple vegetable media.

We believe that our results should recommend the use of a simple vegetable medium for quantitative studies on acidophilus milks because of the ease and simplicity of preparation; because of the characteristic appearance, the sharpness of definition, and size of the colony, and because of the somewhat higher count which is especially marked with the older milks.

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SPECIAL ARTICLES

CURE OF THE CACHEXIA FOLLOWING HYPOPHYSECTOMY BY ADMINISTRATION OF THE GROWTH HORMONE AND ITS RELATION TO THE RESULTING ADRENO-CORTICAL REPAIR[1]

IN 1927, Smith[2] demonstrated that daily intramuscular implants of fresh glandular substance (hypophysis) will repair most,[3] if not all, of the disabilities shown to characterize animals after complete hypophysectomy. The ground was thereby laid for adequate test as to whether extracts of the hypophysis contain one or all of the hormones present in fresh glands. In particular it could be seen, now, whether the growth and gonad-stimulating hormones originally detected by other tests (*i.e.*, their effect on normal mature and immature animals) made up the full complement of anterior hypophyseal hormones, and could therefore completely substitute for the missing gland, and, finally, it should be possible to discover in this way which of the known hormones was responsible for those other features of the syndrome from hypophysectomy besides the disturbance in growth and sex, the deficiency, for instance, in thyroid, parathyroid, and adrenal cortex.

While the administration of extracts, which Evans and Long (1922) had shown to contain the growth hormone, invoked growth in hypophysectomized rats in Smith's hands, he showed clearly that such extracts would not repair the gonadal deficiency, but in fact prevented its repair by implants. He stated that they also did not repair the subnormality of thyroid and adrenal cortex. The present paper reports that the latter result can be accomplished with satisfactory extracts of the growth hormone.

The resumption of growth in hypophysectomized animals following injections of extracts was confirmed by van Dyke. Furthermore, Reichert has shown that the prolonged administration of our growth hormone to completely hypophysectomized puppies produce dogs which exceed in weight and in skeletal dimensions their normal littermate controls. Striking results of this sort have been consistently secured by us. In a long series of experiments on hypophysectomized rats we have administered purified preparations of the growth hormone[4] free from gonad-stimulating effect. The growth rate of hypophysectomized rats receiving these preparations even exceeded that of normal animals receiving the same extracts.

Concerned for some years with the preparation of purified extracts of the growth hormone and more recently with the gonad-stimulating hormone of the hypophysis in order to distinguish their respective rôles or effects, we have applied such extracts to very considerable numbers of hypophysectomized rats. What we desire to place on record in the present communication is the remarkable, prompt and uniform relief from the cachectic state following administration of the growth hormone extracts. Not only is growth resumed, but a rapid and impressive change in the constitutional state of these animals results. This is indicated especially by the resumption of normal turgor to their tissues and above all of normal muscular strength and muscle tone. The myosthenia so characteristic of the hypophysectomized animals disappears within 10 days following the growth hormone therapy. In contrast with these results the administration of even large doses of gonad-stimulating hormone (growth-free) has no influence on the cachexia, even though these products provoke an extreme reaction in the gonads.

The animals injected with growth hormone are indistinguishable from normal animals. Only the genitalia retain the hypophysectomized condition, the glands and accessory organs being atrophic. Autopsy shows repair of both the thyroids and the adrenals. The reparation of the adrenals and thyroids under the influence of growth hormone is, however, not complete. The weights are consistently greater, but do not attain the weight of glands of normal animals. Under the conditions of dosage and time intervals studied thus far, the glands have never attained the weight of those of the normal controls, even when doses are given which stimulate normal or excessive body growth. The gonad-stimulating hormone free of growth hormone, which as stated before has no effect on the cachexia, also does not induce this repair of adrenals or thyroids.

The differences between the histological pictures of the adrenals of hypophysectomized animals and those receiving growth hormone are very striking. In the adrenals of the treated animals the cytoplasm of the cortical cells is restored and three zones become again distinguishable. The gonad-stimulating hormone, on the contrary, produces no such repair in adrenals, even though a dose is administered which causes excessive hypertrophy of the gonads.

The disappearance of the cachectic condition fol-

[1] From the Institute of Experimental Biology, University of California. Aided by grants from the Committee for Research in Problems of Sex of the National Research Council, and from the Rockefeller Foundation. These funds have been generously augmented by the Board of Research and the College of Agriculture of the University of California.

[2] Smith, P. E., *Jour. Amer. Med. Assn.*, 88, 158 (1927).

[3] Normal gestation in the hypophysectomized female was not secured.

[4] These are essentially solutions of the acetone precipitate of the Evans-Simpson-Cornish Extract; see Evans-Simpson-Cornish, *Proc. Soc. Expt. Biol. and Med.*, 1930.

wing the administration of the growth hormone arallels the repair of the adrenals. It is quite prob-ble that the cachectic state and the atrophy of the drenals are causally interrelated and that it is to the hange in the adrenal following administration of the rowth hormone that we must refer the remarkable eneral constitutional restoration. Experiments are ow under way to see whether the cachectic symptoms ollowing hypophysectomy can be relieved, although rowth is not induced, by injections of adrenal cor-ical extracts.

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THE TRANSFORMATION OF SERUM ALBUMIN INTO SERUM GLOBULINS[1]

THE antiprothrombin (heparin of Howell) forms ith protein complex compounds which show differ-nt properties according to the amount of heparin ound to the protein molecule.

The heparin combines with the organic complex of he protein and by means of its affinity for water (lyophil action) increases the solubility of the pro-eins. The antiprothrombin gives the protein an lectronegative charge and moves its isoelectric point towards the acid side.

By adding heparin to pure globulin fraction in alt solution one is able to move the isoelectric point, which for globulins is normally around pH 5.0, to-wards the acid side; the range depends upon the con-entration of the heparin. As is well known, the erum albumin does not flocculate at its isoelectric point. If heparin is added to serum albumin (at pH 5.0), which is freed from globulins by means of dialysis, a flocculation immediately takes place. The degree of flocculation depends only upon the amount of heparin added when excess albumin is present. Albumin-heparin compounds behave as genuine globulins, and we are justified in considering them as identical with the globulins. (Solubility, precipita-tion by means of weak acids, denaturation, precipita-tion by means of salts, solubility in heparin (in ex-cess) dialysis, isoelectric point.)

Experiments on quantitative dialysis of whole erum show that the addition of small amounts of eparin results in an increased amount of globulin (determined by weighing).

The antiprothrombin is so far the only substance normally found in the body (the urea does not play ny rôle in this connection), having the powerful ction described, which is responsible for the forma-ion of the globulins. We found that the amount of globulins present in blood serum is a direct expression of the amount of antiprothrombin. The coagulability of a plasma depends entirely upon the amount of globulins present. By means of the method to isolate the heparin as indicated by Howell, much greater amounts of heparin could be isolated from blood in which the amount of globulin was greater than from any other serums.

From the purified casein (*Hammarsten*) which behaves as a globulin and especially towards heparin, we were able to isolate a heparin-like principle of high activity, if not heparin itself.

Fibrinogen belongs to the globulins. According to the amount of heparin bound to the protein molecule, it is possible to place the various globulins in a certain system. When the various fractions of the serum proteins are precipitated in a buffer system of pH 5.0 as a function of the heparin concentration, the relative charge of heparin on the definite protein fraction may be determined. Heparin acts on the principle of the "unregelmässige Reihe," that is, greater amounts inhibit and smaller promote the flocculation.

The euglobulin goes into solution with a small amount of heparin; the pseudoglobulin needs a larger amount of heparin than the euglobulin for solution. The fibrinogen needs an even greater amount of heparin before it is dissolved. If we consider the serum albumin as heparin free, the other protein fractions of the plasma come as follows: fibrinogen, pseudoglobulin, euglobulin. Our experiments show that the euglobulin fraction is immediately formed when heparin is added to pure serum albumin. We suppose that there exist just as many protein compounds in blood as there are possibilities for heparin-albumin compounds. It is conceivable that the various compounds are dissociated according to the principle of the dissociation of the pluribasic acids. There are just as many dissociation steps as there are possibilities for heparin-albumin compounds, each with its own dissociation constant. This consideration agrees very well with the modern conception (*Sörensen*) which considers the proteins as reversible dissociable systems of components.

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[1] From the Kaiser Wilhelm-Institut für Biologie, Gastabteilung.

PERIODICAL PROPERTIES OF ATOMIC NUCLEI

ACCORDING to our present knowledge of the structure of atoms, there is no reason to expect *a priori* the existence of a simple relation between the electron coverings of an atom and the internal constitution of the nucleus.

Therefore some experimental facts showing such a relation must be of interest. These facts have a statistical character. It is only possible after the investi-

gation of much experimental material to point out the following regularities.

It is well known that the number of nuclear electrons is not a simple function of the atomic weight. There are many isotopes of different elements having the same atomic weight, but different numbers of nuclear electrons.

The number of isotopes having the same atomic weight, N, can be regarded as a function of this atomic weight. Curve 1 in Fig. 1 represents such a function.

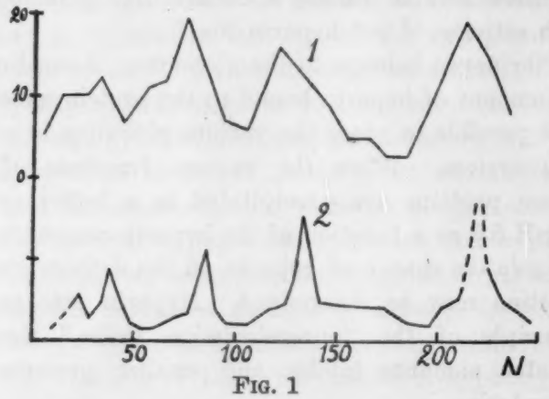

FIG. 1

Each point represents a mean value for an interval of ten atomic weights. It is easy to recognize the periodical character of this curve. It may also be pointed out that the periods correspond to those of the periodical system of the elements. This can be seen comparing curve 1 with curve 2 in Fig. 1. Curve 2 shows also the atomic volume as a function of the atomic weight.

The number of isotopes having the same atomic weight is determined by the properties of the nuclei. On the other hand the atomic volume is determined by the properties of electron coverings of the atom. If we have in both cases the same periodicity we must have also a connection between the outer electrons and the protons and electrons in the nucleus.

There is also another fact showing the same relation. It is known that generally elements having even atomic numbers exist in larger quantities than elements with odd numbers (Harkins' law). This fact can be explained by the greater stability of nuclei with even atomic numbers. Let n_1 be the relative quantity of a given element with an odd number and n_2 the quantity of the next one with an even number. The value

$$E = 1gh_1 - 1gh_2 = 1g\,\frac{n_1}{n_2}$$

can be regarded as a function of the atomic number Z. Taking experimental material from a work of I and W. Noddak[1] it is possible to receive a curve illustrating this function and shown in Fig. 2 (curve 1). This curve can be compared also with the curve of atomic volumes. (Fig. 2, curve 2).

[1] *Naturwissenschaften*, 18, 757, 1930.

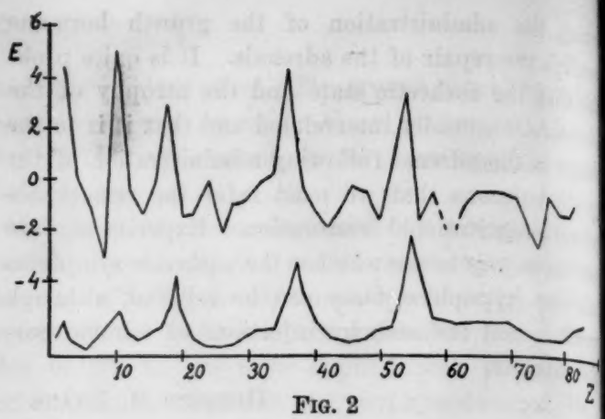

FIG. 2

All the described facts show the existence of some connection between the nucleus and its electron covering. It seems to be probable that quantum laws can be applied not only to the electron covering but also to the nucelus of the atom.

Assuming, (1) that atomic nuclei having the same geometrical structure must have also the same physical properties, (2) that the volumes of nuclei are proportional to the numbers of protons and (3) the protons can not be deformed in the nucleus, it is easy to show that only such nuclei can have the same form as a proton, which have the following atomic weights:

$$N_1 = 1 = 1^3$$
$$N_2 = 8 = 2^3$$
$$N_3 = 27 = 3^3$$
$$N_4 = 64 = 4^3$$
$$N_5 = 125 = 5^3$$
$$N_6 = 216 = 6^3$$

Therefore the atomic nuclei with given values of N must have analogical physical properties. We obtain therefore a periodical system of nuclei, each period limited by N_1, N_2, etc. This is in good agreement with the experimental facts shown in Fig. 1 and Fig. 2.

It is possible also to receive an analogical result from the standpoint of wave mechanics. A more detailed discussion of this possibility will be given in another place.

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ALLUNION ELECTROTECHNICAL INSTITUTE,
MOSCOW 33—AUGUST, 1931

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